PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

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FOR THE YEAR MDCCCX.

PART II.

LONDON,

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PHILOSOPHICAL TRANSACTIONS.

VII. *Supplement to the First and Second Part of the Paper of Experiments, for Investigating the Cause of Coloured Concentric Rings between Object Glasses, and other Appearances of a similar Nature.* By William Herschel, *LL. D. F. R. S.*

Read March 15, 1810.

When the intricacy of the subject, on which my two last papers have been treating, is considered, it will not appear singular that a few supplementary articles should be given. The compression of the account of the experiments into a small compass, where many material circumstances must be left unnoticed, may throw some obscurity on the results, which can only be removed by examining the subject in a fuller extent, and from various points of view. I hope the following illustration and additional explanations will have the effect of clearing up what may possibly to some appear obscure or doubtful, in either the first or second part of my paper, and serve also to make the conclusions, which in the second have been chiefly supported by prismatic experiments, directly

applicable to such as have in the first been made by convex glasses.

That the colours in all prismatic phenomena, which have been examined in the 44th, 45th, 46th, 47th, and 48 articles of my paper, are produced either by the interior critical separation arising from the different reflexibility of the rays which cause the blue bow, or by the exterior critical separation arising from the different intromissibility of the rays which cause the red bow, has been so clearly and circumstantially proved that it can admit of no doubt; it may even be conceived by some that I have been too particular in giving the precise angles, *when we see in the Lectiones Opticæ*, Sect. II. *Par.* 2, *page* 257, 258, *how far Sir* I. NEWTON *has explained the blue bow;* but a sufficient reason for this minuteness was to give greater clearness to my explanation of the new phenomenon of a red bow, which I have with equal precision described, and which by this means may be, step by step, compared with the production of the blue bow. By this precaution I hoped to anticipate any objection that might occur, such as, for instance, that Sir I. NEWTON has *also explained the red bow which* (it may be supposed) *is merely the converse of the blue bow.* This conception, although NEWTON no where speaks of a red bow, seems to be countenanced by what is said after he has shown that the blue bow is caused by the different reflexibility of the rays of light; for as he affirms that the red, orange, and yellow colours are transmitted, he contrives a method of proving it experimentally, by adding a second prism, placed under that which gives the blue bow, and thus making the transmitted rays visible. The full import of this

NEWTONIAN experiment will be considered in the following articles.

LIV. *Supplemental Considerations, which prove that there are two primary prismatic Bows, a blue one and a red one.*

As it will be admitted that we have a primary blue bow, I shall only repeat that by the use of the criterion, which has been indicated at the conclusion of my 44th article, we find that when a plain surface of glass is brought into contact with that side of the prism by which the reflective, or intromissive critical separation is performed, the bow will be turned into streaks, and that the blue bow, which NEWTON has explained, will stand the test of this criterion.

It will now be necessary to prove that the red bow which I have introduced in my 42d article is a phenomenon of equal originality with the NEWTONIAN blue bow. That it will stand the test of the criterion, has already been proved in the 44th article, since by the contact of a plain surface of glass with the efficient surface of the prism this bow is also turned into streaks; but as we find that the NEWTONIAN experiment, by the addition of a second prism, has made the red, orange, and yellow colours, which are the residue of the blue bow, visible, it will be necessary to show that the phenomenon which may thus be viewed is not the red bow I have described.

First consideration. It is a necessary consequence from NEWTON's explanation of the 49th figure in his *Lectiones Opticæ*, page 260, for a copy of which see Plate V. fig. 1; that the angle *t s p* subtended by the transmitted colours must be exactly equal to the angular breadth of the blue bow *t S p* and that also the angular position *C t s*, when the eye sees

the transmitted colours, must be exactly equal to $C\,t\,S$, in which it is to be placed, that the blue bow may be seen. But these angles in my red bow, are not the same as they are in the NEWTONIAN blue one; for, in figure 2, the angle $t\,s\,p$, which the red bow subtends, is $5'\,55'',4$ less than the angle $t\,S\,p$ in figure 3, subtended by the NEWTONIAN blue one; and the position of the eye $C\,t\,s$, in figure 2, for seeing the vertex of the red bow, is $15'\,46''$ less than $C\,t\,S$ figure 3, in which position the eye sees the vertex of the blue bow. Now as these angles arise immediately out of the critical separation of the rays, it is evident that one of these bows cannot be the converse of the other, but that we have two critical separations essentially different, namely, the reflective and the intromissive.

Second consideration. The transmitted colours, which NEWTON makes visible by the addition of a second prism, cannot be seen without it. For, if the red, orange, and yellow rays, were not intercepted by the additional prism, they would be refracted at $p\,r\,t$, figure 1, and pass into the air, scattered in such a manner, as to be totally unfit for giving a distinct image. My red bow, on the contrary, may be seen in one prism, laid down in open daylight, just as we see the blue bow explained by NEWTON.

Third consideration. The residuary colours of the NEWTONIAN blue bow, being transmitted at $p\,r\,t$, the interposition of a second prism, will refract them downwards to s, for which reason, they can never be seen in the form of a red bow, by an eye placed above the prism at S, where the blue bow is visible; whence it follows that, if we were not now acquainted with an original red bow, all the phenomena of the

sudden change of the colours of the bows, which have been explained in my 43d and 45th articles, would still remain unaccounted for.

Fourth consideration. The course of the rays which produce the red bow, is so essentially different from the course of those which form the blue bow, that a mere inspection of the figures which represent them, proves, that one cannot be the converse of the other. The incident rays between A E, α ι, figure 4, are critically separated by the action of the *interior* base; those which are reflected from the space between α and ι, go to H, and form the blue blow; those which are transmitted through the same space, are by refraction scattered over an expansion of 9° 11′ 4″.8 contained between the mean refrangible green, passing from α to g, and the least refrangible red, going from ι to r. The incident rays in figure 5, which enter between A E and α ι, are likewise critically separated, but the cause of this separation is the action of the *exterior* base of the prism; and those which are intromitted between α and ι so as to come to H, produce the red bow; the rest being also intromitted, but not coming to H, are by refraction scattered over a space not exceeding 37′ 7″; the most refrangible violet going from α to v, and the mean refrangible green, from ι to g. As the smallness of some of the angles cannot be accurately expressed in the figures, they may be more correctly compared by the calculated particulars, which are as follows.

	Blue bow.	Red bow.
Convergency of the incident rays	0° 21′ 41″.5	5° 56′ 50″.5
Elevation of the vertices - -	49 57 3.3	49 38 19.5
Divergency of the scattered rays	9 11 4.8	0 37 7

LV. *Illustration of the Dependance of the Streaks of both the Bows, upon the critical Separation.*

One of the reasons, which in the 44th article have been given, for ascribing the colours of the bow-streaks to the critical separation which causes the bows, is their being always in a direction parallel to the bows. With respect to this it may be thought by some, who are still inclined to believe in the fits of easy reflection, and easy transmission, *that streaks parallel to the bows, (though not dependant on critical separation) will in that situation be seen most easily, and most distinctly, because the visual ray in that position passes most obliquely through the stratum of air between the surfaces.* This observation, however, it will be found, cannot be applied to the streaks of either of the bows; for in the 50th article it has already been proved, not that these streaks can most easily, and most distinctly be seen in the place where the bows are, but that they can absolutely not be seen [illegible] ere else.

First illustration. To [illegible] more minutely into a subject which is so essential [illegible] support of the arguments contained in the 52d ar[illegible], let us see whether it will be possible to assign any other reason why the streaks should be parallel to the bows, but their dependence on critical separation? What is there in two plain surfaces that can determine the direction of streaks, supposing they could possibly be formed without depending on critical separation? Why, for instance, should they not be as in figure 6, rather than in figure 7, since in both cases, their arrangement in the shape of a bow, would, according to the objection, be still in the position where the visual ray passes most obliquely through the stratum of

air? The necessity of some cause for the direction of the streaks, may be inferred from the experiment which has been given in the 95th article. For when a plain slip of glass is laid upon a cylindrical curvature, the line of contact, which way soever it be turned, will determine the direction of the streaks that are to be seen; but when two plain surfaces touch all over equally, no bias of this sort can be given to the direction of streaks: the same cause, therefore, which determines the direction of the bow, must also determine that of its streaks, and this establishes their dependence on critical separation.

Second illustration. In what has been said, the possibility that streaks might be formed between two plain surfaces independent of critical separation, has been admitted, but this I cannot allow. The advocates for the colours by thin plates, themselves, must confess, that an uniformly thin plate of air between two plain surfaces, ought not to produce streaks, which contain a variety of colours, so that the very existence of streaks, already proves the action of some principle that will produce different colours; but when the plain side of a prism is laid upon a plain surface of glass, in which situation it has been proved, by the appearance of the bows, that either the reflective or intromissive colour making principle, may be made to exert itself at the interior or exterior base of the prism, and when in either case, streaks are immediately produced, their dependence on the same cause that produces the bows, namely, the critical separation cannot be doubted.

Third illustration. That all the bow-streaks are not only dependant on critical separation, but that each collection depends in particular, on the very principle which forms the

bow to which it belongs, is proved by the characteristic colours of the streaks. In the blue bow-streaks, the blue colour is greatly predominant; and in the streaks of the red bow, the red and green are most abundant, which could never happen if the reflective separation did not copiously furnish the blue, and the intromissive separation as copiously the red and green colours; and what must put this dependance past all doubt, is the sudden change which may be made in the colour of the streaks; for by the mere interposition of a screen, or by lifting up the prism towards the light, we may not only change one bow into the other, as has been proved in the 43d article, but when a plain surface is held under the base of the prism, in order to turn the bow into streaks, we may then change the colours of the streaks belonging to one bow, into those which belong to the streaks of the other, with as much certainty as we can change one bow into the other.

A beautiful experiment to prove this, is as follows. Let two equilateral prisms be tied together as in figure 8. Then standing at the distance of five or six feet from an open window, with the prism held in the situation as represented, the sides *b d*, *d c* being covered with a pasteboard screen *e f g*, look into the side *a b*, straight forward to the window, and you will see beautiful blue bow-streaks. The rays which produce them enter through *a c*, are critically separated *by reflection* from the surface *b c*, and cause the blue bow, which by the plain surface *b c*, of the prism *b d c*, is converted into streaks that go to the eye at E.

Without altering either the position of the eye, or of the prism, drop the screen *e f g*, and the blue bow-streaks will

instantly be changed into those of the red bow. The rays which cause them, enter through the side *b c*, are critically separated *by intromission*, and form the red bow, but are at the same time turned into streaks by the side *b c*, of the prism *b c d*, and go to the eye at E.

The experiment will succeed equally well, if, instead of the prism *b c d*, a highly polished plain slip of glass of the size of the base of the prism *a b c* is tied to it.

LVI. *Illustration of the Dependence of Rings, seen in a Prism upon the critical Separation.*

If it should now be granted, that streaks which may be seen by applying a plain glass to the side of a prism, depend entirely upon critical separation, it may still be doubted, whether the rings which are produced, when a prism is laid upon a spherical surface, are likewise to be ascribed to the same cause, but this may also be decided by a very satisfactory experiment as follows.

Upon a small board, lay a sheet of white paper to reflect light upwards, and through the paper fasten three short tacks into the board; then place an object glass upon the tacks, and put a right angled prism across its surface, which should be of the convexity of a globe of about 30 or 40 feet diameter. A pasteboard screen, formed as in figure 9, must be hung over the vertex to darken the exposed side, that only the scattered light which comes from the paper, may enter the prism through the base, and cause a red bow. The board should be placed upon a stand, near a door which admits the unconfined light of the heavens, where no adventitious colours will disturb the experiment. As soon as the eye

comes to the altitude of the bow, a set of rings will be seen, whose colours, when the bow goes across their center, will be red and green. Some motion of the eye to bring the bow a very little above or below the center, will show the colours to advantage, and in this position of the eye, we are sure to see the rings precisely in the range of the bow which is turned into rings, but remains visible at both sides, where the critical separation is known to take place. When they have been sufficiently viewed, let the screen be removed, that the brighter light of the heavens from above, may transform the red bow into a blue one, which will at the same time instantly change the colours of the rings from red and green to blue.

If it should now be alledged, that streaks or rings may still be independant of critical separations, notwithstanding their taking the colours of the bows, because they must necessarily appear blue, red, or green, when they are seen in rays of these colours, we may answer this objection by proving experimentally, that any adventitious colours that may occasionally mix with streaks or rings, can only tinge them in the places where they pass to the eye in the same direction, but can themselves not produce either blue, red, or green streaks or rings. Place yourself before a window, and holding in your hand a right angled prism, with a plain slip of glass under the base, look in at one side, and turn the prism upon its axis, till you see the horizontal bars of the window tinged with blue above, and red below; bring the red bow-streaks upon one of these bars, and lowering the streaks gradually, you will find that the colours of the bar, merely affect only those parts of the streaks over which they pass, but do not cause any additional streaks of their own colour.

To see this still better, take an equilateral prism with a slip of plain glass held under it as before, and turn the axis of the prism gradually down at the left, till the vertical separations of the panes of glass in the window, are equally tinged with the horizontal ones. Let *a b* and *b c* figure 10, represent one of the angles made by the meeting of the divisions between the panes of glass; then bring the bow streaks into the direction A B, and draw them gradually over the corner *b*, this motion will cause the streaks to be successively cut by the adventitious colours, but you will at the same time observe these colours to remain confined to the individual place over which they pass, and to produce no other effect than what must result from a mixture of their tinge, with the particular colour of the streaks at the place of their meeting. The corner *a b c* will remain perfectly single, which plainly proves that the adventitious colours, not being caused by critical separation, cannot produce streaky phenomena, whereas if they could diffuse themselves, we ought to see at least 5, 6, or 7 coloured angular figures, parallel to each other, as represented at *d e f*.

LVII. *Remarks on Colours supposed to be produced by thin Plates or Wedges of Air.*

First remark. In the 39th article of my paper it has been shown, that coloured appearances, such as streaks, cannot be seen between the plain surfaces of two parallel pieces of glass applied to each other; if an objection should however be made to this, by showing an experiment with two supposed plain surfaces of glass in contact, where irregular streaks, or flashy appearances may be seen, I shall be authorized to

avail myself of what has been proved in the 37th article; for it has been shown, that irregular surfaces will cause irregular figures; for which reason such appearances will only prove, not that plain surfaces can produce them, but that the surfaces between which we see them, are not strictly plain.

Second remark. If it should still farther be conceived that by means of a wedge-formed plate of air, *strait bands of colour would be produced between plain surfaces slightly inclined*, the following experiment will show, that the objection cannot be well founded. I selected two plates of glass, their surfaces being as perfectly plain and parallel as I could possibly find them, and the event shows that they were sufficiently so. The plates were applied to each other in such a manner, that the end of one touched the surface of the other, in a very sharp straight line, while at the opposite end they were kept from contact, by a very fine single thread of the silk worm placed between them, which would produce the required *slight inclination*. No streaks were then visible. I pressed the line of contact strongly together, and streaks became visible; but they were disfigured by pressure, and most disfigured where I pressed most. As soon as the pressure was removed, these coloured appearances vanished. My plain slips were cut with a diamond out of a parallel plate of glass, polished by an optician for optical purposes, and the incumbent slip had its tangent edge finely ground in an angle of about 70 or 80 degrees, to make it a straight line without injuring its plain figure.

Third remark. It will be proper also to take notice of an objection that may be made to the foregoing experiment, by appealing to one of an opposite result; for possibly two plates

of glass, supposed to be plain, may be shown, which, when put together slightly inclined, as the experiment requires, will produce streaks near the line of contact; but, if this should be the case, my 85th article accounts so well for the appearance of such streaks, that it would not be philosophical to ascribe them to plain surfaces, when it has been shown, that cylindrical curves of any figure, will invariably produce them; for which reason, I should think myself justified in concluding, that one or other of the plates, which were supposed to be plain, had a cylindrical termination; the figure of which might be circular, elliptical, parabolical, hyperbolical, or indeed of any other variety of cylindrical curvature.

LVIII. *Illustrating Remarks on the Intention of the 14th Figure, explained in the 48th Article of my Paper.*

The great difficulty of representing rays of light, which are compressed beyond all conception, is such, that even a figure one thousand times magnified, which gives a delineation of them, is hardly less inadequate to give a tolerable idea of what is to be expressed, than if it had not been at all increased in its dimensions. This being the case, it might be expected that some objections would arise, such as that *in my figure constructed for explanation of the streaks the vacancies are observed to correspond with, and to depend upon the intervals between the rays* 1, 2, 3, 4, *&c. originally assumed as separated by blank intervals.* There may appear to be some plausibility in this objection, but still if some notion of this kind should be entertained, it may be shown, not only that such a remark would not be quite correct, but also, that the supposed force of it, is founded on a misconception of the figure.

First remark. When we look upon this figure in a cursory way, it may seem as if the vacancies corresponded with the assumed distances of the rays, but this is partly erroneous; for even in the short extent of the figure, the vacancy between 5 and 6 at the bottom, does not correspond with that at the top. The lower single vacancy at 18, does not agree with the two vacancies above. Between 16 and 17 is a vacancy at the bottom, but none at the top. The lower vacancy at 11, has no corresponding one above; nor has that between 4 and 5 below, a corresponding vacancy at the top.

Second remark. The rays 1, 2, 3, 4, &c. are by no means assumed as separated by blank intervals, but as rays at a certain distance from each other, one thousand times greater in the figure, than in their compressed natural state.

Third remark. The appearance of the rays in my figure, was not intended to represent streaks such as will be seen, but to denote their incipient course in passing from the base of the prism, to an eye whose distance from that base we are to suppose not less than 3000 inches. The visible arrangement and colour of the streaks, erroneously conceived to be expressed in my figure, can only be deduced from the mixture of rays at the place where they enter the eye, the pupil of which it should be remembered, must have a proportional diameter of 200 inches. The angles also, it has been explained, could not possibly be drawn of just dimensions, and from what has been said, we may conclude, that to make a calculation of the mixture and colour of all the rays when they reach the eye, even with the spare quantity of them which has been drawn in the figure, would be extremely laborious, and that a thorough investigation of this particular

point would really, as I have before said, be an endless undertaking. The only fault, therefore, that may be found with my figure, I believe is, that it enters perhaps too particularly into this circumstance. It is well known, that similar discussions about irises, fringes, or halos, have generally been dispatched without giving us the least intimation about the real angular course of the rays that produce them. It was indeed, not incumbent on me to go so far, but where angles and distances and intersections fell in my way, that could be determined, I was unwilling to pass them by unnoticed, as such considerations certainly tend to facilitate our conception of the ultimate production of the streaks.

Fourth remark. That such delineations may be used, to show in what manner we may conceive intricate optical phenomena to be produced, I have the authority of eminent writers in my favour; thus NEWTON, in his first figure of the 3d book of Optics, assumes four rays on each side, to illustrate in what manner we may conceive that a hair can give a proportionally larger shadow near its body, than at a distance; but no one will affirm, that these four rays can give an idea of the actual quantity of light, and the real angles in which it falls on the different parts of the paper; all which it would be necessary to show, in order to prove, that the appearance of it on paper, agrees with the hypothesis; and yet we may nevertheless perfectly well conceive the author's meaning, and can make no serious objection to his explanation, on account of his having taken but four rays, at four arbitrary distances, moving in four arbitrary angles.

LIX. *Experiments on the multiplying Power of Surfaces in contact, which modify the Form of prismatic Appearances.*

The simplicity of the following experiments is such as will ensure them a ready admittance, even by those who may not have an opportunity of repeating them; their application also to some of the most intricate phenomena of modifying the form of the prismatic appearances, must render them of considerable value.

First experiment. Upon a plain metalline mirror 5¼ inches long, and 4 broad, I laid the base A B C D, figure 11, of a right-angled prism; and having darkened the room, a candle was placed so as to throw its light upon one side of the prism, the reflection of which from the base, I saw through the other. The eye was then gradually lifted up to such an altitude, that a blue bow, if it were made visible by the admission of an uniform scattered light, would extend from *a* to *b*; the candle was then withdrawn, till only the inverted flame of it remained visible at *c*. A small pasteboard screen as long as the prism, and bent at the top as in figure 9, must be hung by the end *a* upon the vertex of the prism, to cover the reflected image of the candle, and the side *b c* must be short enough to leave about one or two tenths of an inch open for scattered light to enter, so as by reflection from the mirror, in proper angles to make the blue bow visible. Every thing being in this arrangement, and the room properly darkened, as well as the eye guarded from the direct light of the candle, place the pasteboard screen on the prism, and you will then perceive a very bright small spectrum of red and green light at *d*, which consists of those rays that in the blue bow place are

transmitted through the base of the prism, and are reflected by the mirror in such a direction, as to come to the eye. I do not mention the blue bow streaks which may be seen with some attention; they are very faint, and are not the object of this experiment, serving only to prove, that the coloured spectrum is formed in the bow place.

Second experiment. Every thing remaining arranged as before, lay a narrow slip of thin pasteboard *e f* under the end of the prism at A C, figure 12, but leave B D in contact; the eye must also be elevated till the bow place comes up to *a b*. In this position you will see the small image multiplied, so that, according to the brightness of the candle, and clearness of the prism and reflector, 6, 7, or 8 coloured spectra may be perceived, arranged from *d* to *g* as expressed in the figure. They are not perfect images of the candle, but so many reflections of the red, yellow, and green light transmitted at the blue bow place, and every one of them will accordingly be seen to be nearly as broad in the green part, as in the red; none of them coming to a tapering point, like the white image of the candle. The spectra are consequently occasioned by a reiterated reflection of the critically separated rays between the subjacent mirror, and the exterior surface of the base of the prism. Indeed, nothing can be more evident than this reiteration of reflections, which is so well known, that opposite parallel mirrors are often put up in rooms, to produce a multiplied extent.

Third experiment. When the eye is lifted high enough to have the line of the critical reflection quite above the prism, and the small screen is also taken away, we may repeat lifting and depressing the end A C, and the two reflecting surfaces

will then, by reiteration in the common way, give us a set of complete tapering images of the white flame of the candle, arranged as in figure 19. And in consequence of the various distance of the prism from the mirror, the distance of the several images of the candle, will increase and decrease, so that when at last, the end A C is again set down on the mirror, they will apparently coalesce into one single bright image.

The use that may be made of these experiments is as follows. From the laws of reflection we know, that the extent of the multiplied images perceived in reiterated reflections, between two surfaces, may at all times be ascertained when their distance is given. It is also well known, that when two plates are in what is called contact, we can in fact, only suppose them to be extremely near each other. The production of streaks, when a plain glass is laid under a prism, is a sufficient proof that, even when they are in the closest contact which can be made, the subjacent surface still acts by reflection upon the rays that pass through the base of the prism; for if the contact of the two surfaces were so complete as to make one solid mass of glass, no reflection would take place within its substance.

LX. *Of the breadth of the Streaks compared to that of the Bows, and the Cause why they must take up a broader Space than the Bows from which they are derived.*

It must have been noticed by those who have examined the beautiful streaks, which in my paper it has been shown, will be produced when a plain surface is held under the base of the prism, that they take up a broader space than the bows;

and notwithstanding what has been said in explanation of their production, by the reflection of the transmitted rays in the 14th figure of the second part of my paper, it may probably be objected, *that since the space occupied by the streaks adjacent to the bows, is much broader than either of the bows, it is conceived, that critical separation will not account for them.* At a first view of this remark, it may in some measure appear to be justified; for if there were no other cause than merely critical separation, the increase of the breadth of the streaks could not well be accounted for. It must however be recollected, that according to what has been proved in the 47th and 48th articles, the modifying power of surfaces in the production of streaks, is added to the principle of the critical separation which produces the colours.

First cause. In consequence of the reflection of the transmitted rays, from the plain surface held under the prism, it may already be seen in the abovementioned 14th figure, that the streaks must take up a greater extent than the bows; for instance, the last reflected colour which is marked, enters the prism again at *v* 2,87 inches beyond the faintest part of the bow. This gives a magnified extent of 27,44 to the streaks, that of the bow being 24,8.

Second cause. But in order to find a greater correspondence between calculation and actual observation, I must repeat that we are not to suppose the twenty intervals between the assumed rays to be blanks. The reason why more rays were not introduced, was to avoid crowding the figure unnecessarily; but let us take, for instance, blue rays falling on the inferior base at ,009 of an inch from β towards No. 4; then, in order to be reflected so as to reach the eye, they must have the

oblique incidence of 49° 49′ 21″; and since we know that many of the rays which fall on the same spot, will be transmitted while others are reflected, we find by computation, that blue rays with the above incidence being transmitted, will be refracted in such a manner, as to arrive at the subjacent reflecting surface in an angle of 11′ 32″, and will, therefore, re-enter the prism at the magnified distance of 59,6 inches from the place at which they left it, or 63,2 from *a*; and this will give an extent to streaks, amounting to nearly $2\frac{1}{3}$ times the breadth of the bow. The same will take place with indigo and violet rays to an indefinite extent, which it will not be necessary to particularise.

Third cause. In the foregoing article it has been shown, that beside the single reflection, which in the 14th figure is delineated as taking place between the base of the prism, and the subjacent reflecting plane, 6, 7, and 8 succeeding reiterations of the same effect will carry on the reflected rays to a certain extent which is assignable. These rays would have crowded the figure so much, that they could not be inserted; but let us see how far they may extend the breadth of the streaks. I have already shown, that the first reflection, on the magnified dimensions of the figure, will carry the transmitted rays 2,64 inches beyond the bow; these rays, by reiterated reflections, may therefore be extended to 14,84; 18,48, or 21,12 inches beyond it, which alone will be sufficient; but if moreover, the intermediate rays are here also taken into consideration, there cannot remain a doubt, but that the breadth of the streaks is sufficiently accounted for.

LXI. *Of the Manner in which Rays that are Separated by critical Reflection or Intromission come to the Eye.*

The subject of vision in general, affords so many intricate phenomena, that we must not be surprised if some things occur, that are of difficult conception. By means of the principle of the intromissive separation of the colours, I have already accounted for several appearances, that no other principle, not even the NEWTONIAN fits of easy reflection, and easy transmission can possibly reach. If therefore, it should be objected to my ascribing the generation of the colours of the NEWTONIAN rings, likewise to critical separation *that rings must arise from some other cause than critical separation, because they can be seen at the under surface of a glass terminated by parallel planes, (as in figure 14,) and in other situations in which critical separation cannot reach the eye*, it will be necessary to examine how far this observation is well founded.

The objection seems to convey a double assertion; the first is, that in the situation of a plain glass laid upon a convex surface, no critical separation can reach the eye; and the next, that if I cannot show how the rays come to the eye, the rings cannot be caused by critical separation. The first of these positions contains something taken for granted, which cannot be admitted. It supposes that I affirm the critical separation to be the sole cause of the rings, whereas I have plainly shown, that this separation furnishes only the colours, and that the modifying power of the subjacent spherical reflecting surface, turns these colours into rings. Now to show how very different an effect may be produced, when the critically separated rays are acted upon by the modifying power

of a surface upon which they fall after their separation, I need but refer to the experiment which has been mentioned in the 5th paragraph of the 42d article, where the rays of the bows, which can only be seen in one particular situation, when they come directly to the eye, are effectually rendered visible in every direction, by the scattering power of the surface on which they are thrown.

I proceed now, by different decisive experiments to prove, that the objection in neither of the senses it may be taken, can affect the validity of the theory I have explained.

First set of experiments. Having ground and polished a metalline mirror, to the convexity of a sphere of 40 feet diameter, I laid upon it a right angled prism, and when they were properly exposed to the light, I lifted the eye gradually up to the blue bow place, and saw the rings that were formed of the colours critically separated by the base of the prism. That these rings owe their formation to the joint effect of the critical separation, and modifying power of the spherical metalline reflecting surface, cannot, after what has been proved in the 56th article, admit of a doubt. I then lifted the eye very slowly higher and higher, till it was brought to the vertex of the prism, and attending minutely to the rings all this time, I could no where perceive the least interruption in their uniform visibility. I do not take notice of the gradual changes in the colour and size of the rings, because such gradual changes equally happen to those that are seen between object glasses. When the eye is over the vertex, the prism being equiangular at the base, we see in the opposite side an equal set of rings. We may then advance the eye still farther, and keep the first set in view, or, what will be more convenient,

take up the opposite set of rings, and confining our attention to it, may draw the eye down again till we lose it: which will not be, till when the eye is nearly brought down to the level of the side exposed to the light. Here then we have an instance of rings composed of the colours furnished by critical separation, which may be seen from the obliquity of 82° 17′ 31″, down to I suppose about 5 degrees, which gives an angular space of at least 77 degrees.

To extend this range farther, I used several prisms with different refracting angles; first, one where that angle was 30 degrees; then one with 25; another with 20; and also one of 9 degrees. By this successive change of prisms, it was ascertained, that the range of visibility increased, when a smaller refracting angle was used. In the last prism, the rings became visible at an elevation of 36° 43′ 6″; this up to 90 on one side, and down again at the other to 5 degrees, gives a range of more than 198 degrees, in which these rings may be seen. To manage this experiment it is necessary, when the eye is vertical, gently to turn the mirror with the prism upon it half round, that the eye may then be depressed gradually, without interfering with the incident light.

From these experiments it may be presumed, that were the refracting angle still farther diminished, it would increase the range at last to that of a plain glass, which therefore, I am authorized to look upon, as a prism with a vanishing refracting angle. It will be seen presently, that even this has been completely verified.

As the modifying power of spherical surfaces, to render critically separated colours visible in every direction, is by these experiments established, we might take it for granted,

that a similar power will be exerted by all sorts of curvatures and irregularities of reflecting surfaces; but in order to take nothing upon trust that may be proved, I had recourse to the following experiments with different curvatures.

Second set of experiments. Upon a ridge of glass ground and polished to a cylindrical form, I laid the base of a prism, with one angle of 96; and two of 42 degrees each. In this I saw beautifully coloured streaks, or rather very narrow lenticular configurations, on one side as low as the angle of 42°, will allow the critical separation to be seen, and on the other down to within about 5° of the level of the plane, which gives a range of visibility of nearly 82 degrees.

Having also provided a refracting angle of 3° 48′ 8″, and lain it upon the same cylindrical ridge, the visibility of these phenomena was extended to 152 degrees.

A plain glass laid upon the same ridge, extended this range to 170 degrees.

That these effects of extending the range of the angular space, in which the narrow coloured lenticular forms are visible, is owing to the modifying power of the cylindrical surface, is particularly evident from the parallelism of the coloured phenomena with the line of contact, and from the direction of their extended visibility at right angles to this line.

Having laid a right angled prism upon the same ridge, I perceived not only the coloured primary figure, but also a similar, magnified secondary one, which I ascribed to a reflection from the flat base of the glass, the upper side of which, contained the polished cylindrical ridge. To take off this secondary image, I deprived the base of its reflecting

power, by rubbing it on emery, which had the required effect.

A doubt might occur, with regard to the cause which produced the colours, because in these experiments the subjacent medium was of glass; and although on account of the emeried base, we may be certain that the curved ridge only acted by reflecting the critically separated transmitted colours in a variety of angles, yet to prove in the most satisfactory manner, that these colours were exclusively from the prism, I ground and polished two metalline cylindrical pieces to different curvatures, and laying upon them my small prismatic angle of 3° 43′, the minute lenticular figures, became again visible over an extent of 152 degrees.

To vary the experiment, I placed a cylindrical ridge in contact with a plain metalline mirror, and saw with great facility through the plain side of the glass which was towards the eye, and was highly polished, the extended range of the modified colours, which in this case amounted to 170 degrees.

From this variation a very important consequence may be drawn, which is, that a radiation from curved surfaces reflected by plain ones, will effect the same extension of visibility, as a radiation from plain surfaces reflected by curved ones. How much more then must this effect be produced, when two curved surfaces are applied to each, as for instance, when two double convex object glasses are laid together?

Third set of experiments. Upon a piece of mica tied over a cylinder, I placed a right angled prism, and having brought the eye to the altitude where critically separated rays are visible, I perceived a number of irregular forms of beautiful colours. When the position of the eye was gradually changed,

I found that the modifying power of the irregularly curved surface of the mica, made these configurations visible over the same angular space in which I had already seen the rings, and lenticular figures.

These appearances are often so delicate, that they may easily escape our notice; although we should follow them with great attention when the eye is moved. This will happen, especially when they are extremely minute, and in the experiment of the last paragraph of the 50th article, I had actually overlooked them; but on repeating the same afterwards with a magnifier, I perceived them without much difficulty.

I tried not only the smaller angles of the former experiments, with the same result of a gradually increased range of visibility, but had also recourse to the plate of glass of unequal thickness mentioned in the 32d article, the sides of which, when produced, would meet in an angle of 2′ 2″. Its surfaces approach so nearly to parallelism, that the inference I had drawn from the trial of smaller angles, when rings on the convex mirror were examined, was now verified by an application of this plate to the surface of mica; for with the assistance of a magnifier, I saw the coloured forms over an angular space amounting to 171° 18′ 28″, which is full as much as we could have seen with a plain glass. From this range, in which the actual angles of elevation of the eye above the plane of the glass at each extreme were measured, it appears that 5 degrees, which I have before allowed for this purpose, is more than sufficient.

The foregoing three sets of experiments prove, that the first of the assertions, into which I have divided the objection,

is not well founded, because the modification of the subjacent reflecting surface, so essential to the formation of the phenomena under consideration, has not been attended to. In addition to this, the inference I have drawn from a foregoing experiment proves, that not only the modification of the reflecting surface, but also that through which the rays are transmitted and radiate upon the subjacent one, must be equally taken into consideration.

It remains therefore established, that by combination, the figure of either of the surfaces in contact, be it the reflecting or the radiating one, will make these appearances visible over an extended space, in the shape of rings, ellipses, lenticular figures, and all sorts of irregular configurations, except in the only case, where both reflection and radiation, happen between two plain surfaces in contact, and where consequently no change in the angle of seeing the critically separated rays can take place. The uniformity of this modification will then produce streaks, only visible in the bow place.

If the objection should now assume the second form, which is, that unless I can show how the rays of the critical separation thus modified can reach the eye, the rings must arise from some other cause, I may then fairly say, it is sufficient to have proved two very essential points, the first of which is, that these rings are formed from the colours of the critical separation, modified by the subjacent reflecting surface; and the next, that when this modification is caused by subjacent, or even by incumbent surfaces of any curvature whatsoever, that can be brought into proper contact, their modification of reflection or radiation will then increase the field of visibility of all the various coloured phenomena that can be produced,

to whatever extent the circumstances of the combination of the two essential surfaces may allow. The very case proposed in the objection, of a slip of plain glass laid on a spherical surface, has been examined in the most simple form possible, by using for the spherical curvature, a piece of polished metal, to exclude all adventitious source of the generation of colours, and employing for the contact of a plain surface, the base of a prism, on the inside of which, according to the NEWTONIAN doctrine of the different reflexibility of light, it must be admitted, the colours will be critically separated. Then, without the least change of form, or contact of the two essential surfaces, it has been proved, that a diminution of the prismatic angle, will gradually extend the visibility of the rings, till, even before that angle comes to a vanishing state, where the prism would be converted into a plain slip of glass, the rings may be seen in every direction, and at any elevation of the eye, in which they can be seen, through a slip of glass such as the objection supposes. As this then has been proved not only of rings, but of all other possible configurations, that may be caused by the rays of the critically separated colours, modified by reflection or radiation from curved surfaces, it is evident, that an objection which asserts that such colours cannot be seen, contradicts the plainest and best established facts.

With regard to the actual course of the rays from the very moment of their critical separation into colours, till they produce the required effect, it cannot surely be expected that I should trace them through a most intricate complication of reflections from curve to curve, when it has been shown, in the second part of my paper, that even with streaks, which

Fig. 1.

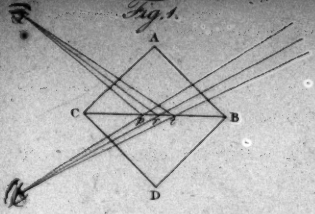


Fig. 2.

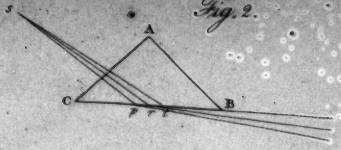


Fig. 4.

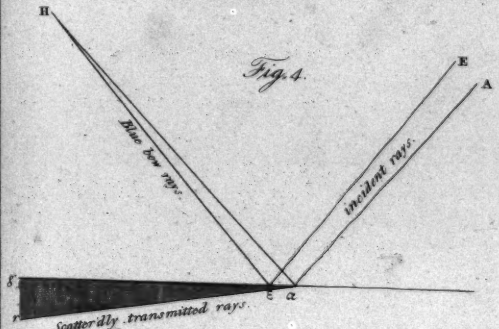
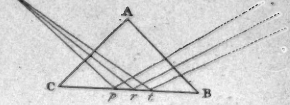


Fig. 3.



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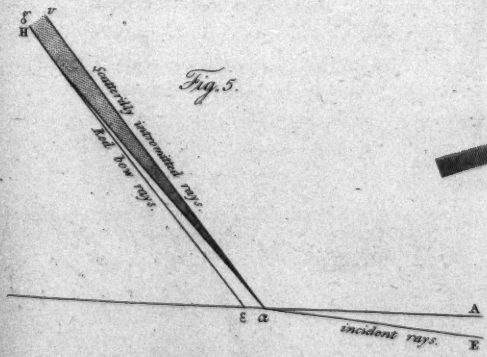


Fig. 6.

Fig. 7.

Fig. 8.

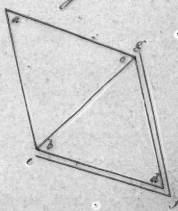


Fig. 9.

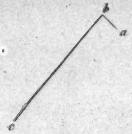


Fig. 10.

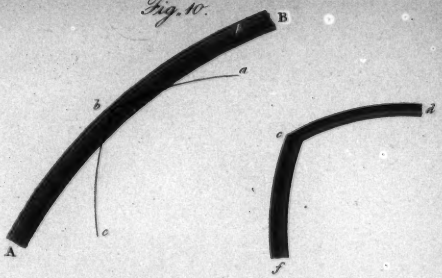


Fig. 11.

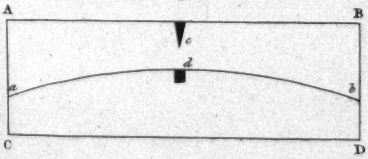


Fig. 12.

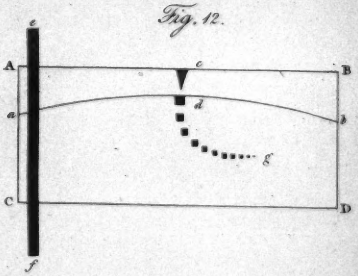
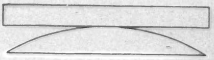


Fig. 13.

Fig. 14.



are produced by the contact of two plain surfaces, it would be an endless undertaking to follow them till they enter the eye. Enough has already been said upon this subject to convince every intelligent reader, that all the phenomena of coloured rings, which have been ascribed to the effect of certain fits of easy reflection and easy transmission of the rays of light, as well as the great variety of other coloured appearances of which I have treated, admit of a most satisfactory solution, by substituting the solid principle of the critical separation of the different colours in the room of these fits.

VIII. *On the Parts of Trees primarily impaired by Age. In a Letter from* T. A. Knight, *Esq. F.R.S. to the Rt. Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.*

Read March 22, 1810.

MY DEAR SIR,

IN the first communication I had the honour to address to you, (it was in the year 1795,) I stated the result of many experiments on grafted trees, from which I inferred that each variety can be propagated with success, during a limited period only; and that the graft, or other detached part of an old tree, or old variety, can never form that, which can with propriety be called a young tree.

I have subsequently endeavoured to ascertain which, amongst the various organs that compose a tree, first fails to execute its office, and thus tends to bring on the incurable debility of old age; and the result of the experiments appears sufficiently interesting, to induce me to communicate an account of them to you.

Whatever difference exists between the functions of animal and vegetable life, there is a very obvious analogy between some of the organs of plants, and those of animals; and it does not appear very improbable, that the correspondent organ, in each, may first fail to execute its office; and satisfactory evidence of the imperfect action of any particular

organ can much more easily be obtained in the vegetable, than in the animal world. For a tree may be composed, by the art of the grafter, of the detached parts of many others; and the defective, or efficient, operation, of each organ, may thus be observed with the greatest accuracy. But such observations cannot be made upon animals; because the operations necessary cannot be performed; and therefore, though there would be much danger of error in incautiously transferring the phenomena of one class of organised beings to another, I conceive that experiments on plants may be, in some cases, useful to the investigator of the animal economy. They may direct him in his pursuits, and possibly facilitate his enquiries into the immediate causes of the decay of animal strength and life; and on a subject of so much importance to mankind, no source of information should remain unexplored, and no lights, however feeble, be disregarded.

Naturalists, both of ancient and modern times, have considered the structure of plants, as an inversion of that of animals, and have compared the roots to the intestines, and the leaves to the lungs, of animals; and the analogy between the vegetable sap, and animal blood, is very close and obvious. The experiments also, of which I have at different periods communicated accounts to you, supported by the facts previously ascertained by other naturalists, scarcely leave any reasonable grounds of doubt, that the sap of trees circulates, as far as is apparently necessary to, or consistent with, their state of existence and growth.

The roots of trees, particularly those in coppices, which are felled at stated periods, continue so long to produce, and feed, a succession of branches, that no experiments were

wanted to satisfy me, that it is not any defective action of the root which occasions the debility and diseases of old varieties of the apple and pear tree; and indeed experience every where shows, that a young seedling stock does not give the character of youth to the inserted bud or graft. I, however, procured plants from cuttings of some very old varieties of the apple, which readily emit roots; and these plants at the end of two years were grafted, about two inches above the ground, with a new and very luxuriant variety of the same species. These grafts grew very freely, and the roots themselves, at the end of four or five years, probably contained at least ten times as much alburnum, as they would have contained, had the trees remained ungrafted. The roots were also free from every appearance of disease, or defect.

Some crab-stocks were at the same time grafted with the golden pippin, in a soil where the wood of that variety rarely lived more than two years; and I again grafted the annual shoots of the golden pippin, with cuttings of a young and healthy crab tree, so as to include a portion of the wood of the golden pippin, between the roots and branches of the native uncultivated species, or crab tree; and in this situation it grew just as well as the wood of the stock and branches. Some branches also of the golden pippin trees, which I mentioned in my former communication of 1795, being much cankered, were cut off about a foot above the junction of the grafts to the stocks, and were regrafted with a new and healthy variety. Parts of the wood of the golden pippin, in which were many cankered spots, were thus placed between the newly inserted grafts, and the stocks; and these parts have subsequently become perfectly free from disease, and

the wounds, previously made by canker, have been wholly covered with new and healthy bark. These facts, therefore, satisfied me, that the debility and diseases of old varieties of fruit of this species, did not originate in any defective action of the bark or alburnum, either of the root, or of the stem and branches, and my attention was consequently directed to the leaf and succulent animal shoot.

A few crab-stocks were grafted with cuttings of the golden pippin, in a situation and soil, where I had previously ascertained that the wood of the golden pippin rarely remained in health at the end of a second year; and, as soon as the annual shoots had acquired sufficient growth and firmness, numerous buds of a new and luxuriant variety of apple, which had recently sprung from seed, were inserted in them. During the succeeding winter, the natural buds of the golden pippin branches were destroyed, and those inserted suffered alone to remain; and as soon as the leaves of these had unfolded, and entered on their office, every symptom of debility and disease disappeared in the bark and wood of the golden pippin; and each continued to perform its office, just as well as the wood and bark of the young seedling stocks could have done under similar circumstances. I made nearly the same experiments on the pear tree, and with the same result.

I have endeavoured, in several former communications, to prove that the sap of plants circulates through their leaves, as the blood of animals circulates through their lungs; and I have not subsequently found any facts, in the writings of other naturalists, or in my own experiments, which militate against this conclusion. I have also observed, that grafted

trees, of old and debilitated varieties of fruit, became most diseased in rich soils, and when grafted on stocks of the most vigorous growth; which has induced me to suspect, that in such cases more food is collected, and carried up into the plant, than its leaves can prepare and assimilate, and that the matter thus collected, which would have promoted the health and growth in a vigorous variety, accumulates, and generates disease in the extremities of the branches and annual shoots, whilst the lower part of the trunk and roots remain, generally, free from any apparent disease. I am, therefore, much disposed to attribute the diseases and debility of old age in trees, to an inability to produce leaves, which can efficiently execute their natural office; and to some consequent imperfection in the circulating fluid. It is true that the leaves are annually reproduced, and therefore annually new: but there is, I conceive, a very essential difference between the new leaves of an old, and of a young variety: and in support of this opinion, I shall observe, that the external character of the leaf of the same variety at two, and at twenty years old, is very dissimilar; and it therefore appears not improbable, that further changes will have taken place at the end of two centuries.*

If these opinions be well founded, and the leaves of trees

* The leaf of a seedling apple or pear-tree, when the plant is very young, is generally almost wholly free from the pubescence or down, which subsequently appears on its under surface; and which BONNET and M. MIRBEL, have supposed to increase its surface and powers. But I feel little disposed to adopt this hypothesis, having observed that the leaves of some new varieties of the apple, which have sprung from seeds of the Siberian crab, have both surfaces nearly equally smooth; and that these varieties grow faster, and bear heavier crops of very rich fruit, than any others, without being exhausted or injured.

be analogous to the lungs of animals, is it very improbable that the natural debility of old age of trees and of animals, may originate from a similar source?—This is a question, upon which I am not by any means prepared to give an opinion: but I believe it will very generally be admitted, that the human subject is best formed for long life, when the chest is best formed to permit the lungs to move with most freedom. I have also long and attentively observed amongst our domesticated animals, that those individuals longest retain their health and strength, and best bear excessive labour and insufficient food, in which the chest is most deep and capacious, proportionately to the length of current the circulating fluid has to run; and the same remark will, I believe, be generally found applicable to the human species.

I am, my dear Sir,

with great respect,

sincerely yours,

Downton, Feb. 26, 1810. THO. AND. KNIGHT.

IX. *On the Gizzards of Grazing Birds.* By Everard Home, *Esq. F.R.S.*

Read April 4, 1810.

In the course of my enquiries into the various modes in which the food in different animals is prepared for digestion, many circumstances have been met with, which tend to shew, that grass is the substance of all others, on which animals feed, that requires the most preparation; and that ruminating animals are fitted by nature in an eminent degree, for extracting the greatest possible quantity of nourishment from this species of food.

If we examine the means employed by quadrupeds of this description, it will appear, that the grass in the first instance is broken short off from the ground and swallowed; then macerated with the mass that had previously undergone some preparation in the first stomach, and afterwards a part of the whole of this mixture, before it is digested, is brought back into the mouth, there masticated in a particular manner; not as in the elephant and horse, or those nearest allied to them, but by being obliquely cut into smaller portions and then mixed up with the saliva into pellets, which are conveyed through the third stomach into the fourth, for the purpose of digestion. This is a mode of preparation peculiar to ruminants, no other quadruped being provided with all the same means.

Finding so marked a peculiarity in all the organs connected

with digestion, in those animals who naturally live wholly on grass, I was led to consider that as some birds graze, there must in them be peculiarities in the form of their gizzards, to fit them for digesting this particular kind of food, by which they may readily be distinguished from the gizzards of other birds.

With this view, I examined the gizzards of the goose and swan, who are both in the habit of grazing, (though geese more constantly so,) and compared their structure with that of the turkey, a bird nearly of the same size, whose food is of a different kind.

To make this comparison more correct and satisfactory, a goose's and a turkey's gizzard were moderately filled with plaister of Paris, and then boiled; by this means the cavity was kept open, and the course of the muscular fibres very readily traced.

In the turkey, the external surface was first attentively examined, viewing that side which is anterior in the living bird, and on which the two muscles and middle tendon are most distinct, there being no other part to obstruct the view; while on the opposite side there is the opening of the pylorus, and the bag formed by the lower portion of the gizzard beyond the circumference of the muscles.

In this view, the muscle on the left side is distinctly larger than that on the right, as will be seen in the annexed drawing. This appears on reflection to be a great advantage in producing the necessary motion; for if the two muscles were of equal strength, they must keep up a greater degree of exertion than is necessary; while in the present case, the principal effect is produced by the left side, and a smaller force is used by the right, to bring the parts back again.

The muscles when dissected, are found to be composed of fasciculi of fibres, connected together by a loose cellular membrane, so as to appear like so many separate muscles lying parallel to each other. They all arise from the anterior strong flat tendon, and are inserted into that on the posterior surface; as they are very numerous and require a large surface of insertion, part of them, on a superficial examination, appear to be lost on the internal membrane of the gizzard.

These muscles by their alternate action produce two effects; the one a constant friction on the contents of the cavity, the other a pressure upon them. This last arises from the swell of the muscle inwards, which readily explains all the instances which have been given by SPALLANZANI and others, of the force of the gizzard upon substances introduced into it; which force is found, by their experiments, always to act in an oblique direction.

The internal cavity, when opened in this distended state, is of an oval form; the long diameter in the line of the body: its capacity nearly equal to the size of a pullet's egg, and there are, laterally, ridges in the horny coat, in the long direction of the oval.

When the horny coat is examined in its internal structure, the fibres of which it is formed, are not found in a direction perpendicular to the ligamentous substance behind it, but in the upper portion of the cavity they have a direction obliquely downwards, and in the lower portion obliquely upwards.

From this form of cavity it is evident, that no part of the sides are ever intended to be brought in contact; and that the food is triturated by being mixed with hard bodies, and acted on by the powerful muscles which form the gizzard.

In the goose, the gizzard differs very much in its external form, from that of the turkey; being oval in its transverse direction, and having the lateral edges very thin; this arises from the fasciculi of fibres of both muscles being more numerous than in the turkey. The disproportion between the left muscle and the right, is still greater than in the turkey, and when the cavity is opened it is flat, divided into two equal portions; that on the right side convex above, forming a projection into the cavity, and concave below, forming a hollow. On the left side there is a corresponding surface, only that the concavity is above, answering to the convex part opposite; and a convexity below, fitted to the opposing concavity.

The horny covering of these surfaces is very strong, much more so than in the turkey, and when the structure is examined, the direction of its fibres is oblique; on the right side from above downwards, and on the left side from below upwards.

In the swan, the gizzard is altogether larger than in the goose, and more flattened at the external edge, so that the muscular fibres are shorter; on its internal cavity the appearances are nearly the same, but there is a less strongly marked ridge and concavity, and the surface of the oval portion, in which these are met with, is smaller in size. The substance of that part of the gizzard appears to be less dense, but the cuticular covering is thicker, and its fibrous texture more conspicuous. The direction of the fibres is exactly the same as in the goose.

From this construction of cavity it is evident, that these two corresponding surfaces are intended to move on one another,

with little more than the food between them; the hard substances interposed, being only of a small size, so as not to interfere with their regular correspondence with each other.

In these gizzards there is not the rotatory motion mentioned in the turkey, but a regular sliding motion, begun by the strong muscle forcing one side of the horny substance over the surface of the other; and it is brought back by the action of the weaker.

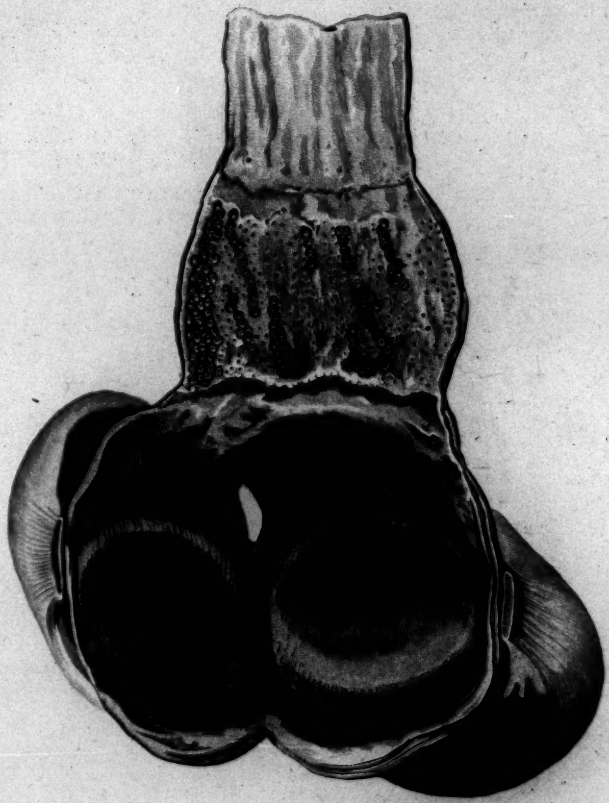
This motion bears so great a resemblance to that of the grinding teeth of ruminating animals, in which the teeth of the under jaw slide upwards, within those of the upper, pressing the food between them, and fitting it by this peculiar kind of trituration for being digested; that it is fair to conclude, it is for the same purpose, more especially, when we only find it in those birds that graze.

The gizzard of the goose is evidently fitted for a harsher kind of grass than that of the swan, and we find that the goose prefers the common grass of the fields, while the swan is partial to the soft weed found in ponds and rivers, and only grazes occasionally.

The bill of the goose breaks off the grass short from the ground, nearly in the same manner as the fore teeth of ruminants. In like manner as ruminants have only teeth in the under jaw, so has the goose small pointed cuticular teeth in the lower portion of the bill, which, when the bill is closed, fit into grooves in the upper one, so that the grass inclosed in the space between the teeth and grooves, is nipped off.

There is a swell in the lower part of the œsophagus in these birds, which is peculiar to them: this answers the purpose of a reservoir, in which the grass is retained, macerated,

Fig. 2.



W. Clift del.

P. Basire sc.

and mixed with the secretions poured out by the glandular surfaces surrounding it, in this respect corresponding to the first and second stomach of ruminating animals, in which the grass is prepared for mastication.

These facts, as they account for the goose and swan living upon a species of food not fitted for birds in general, proves that in this class of animals, a peculiar conformation of the digestive organs is required, as well as in quadrupeds, when they are intended by nature to live intirely upon grass.

This fact being ascertained, is not without its importance in elucidating this branch of physiological inquiry.

EXPLANATION OF THE PLATE.

Fig. 1. The gizzard of a turkey laid open on its anterior part, to shew the form of the internal cavity, which is oval, and the grinding surfaces uniformly concave.

Fig. 2. The gizzard of a swan exposed in the same way; the grinding surfaces have an oval form, but in an opposite direction to those of the turkey, and each of them is made up of a ridge and a hollow in the direction of the oval, which are adapted to those of the opposite side, the ridge of the one fitting the hollow of the other.

X. *Observations on Atmospherical Refraction as it affects astronomical Observations; in a Letter from* S. Groombridge, *Esq. to the Rev.* Nevil Maskelyne, *D. D. F. R. S. Astronomer Royal. Communicated by the Astronomer Royal.*

Read March 28th, 1810.

DEAR SIR, *Blackheath, 22nd January*, 1810.

THE great advantages we derive from the labours and writings of those learned men, who have preceded us; the profound researches in physical astronomy by the mathematicians of the last century; and the improvements in the construction of instruments, effected by the ingenuity of our modern artists: these circumstances should stimulate all those who have inclination and opportunity, to the endeavour of contributing to the common stock of knowledge. I beg leave to add my tribute of admiration to the general voice, for the large share you, Sir, have taken in the promotion of science, not only by your learned publications, but also by the liberal encouragement and assistance you have given to those who have been honoured with your acquaintance. I am, therefore, induced to commit to your protection the inclosed paper, acknowledging myself indebted to your friendly communications for much useful information. I could have wished the task had been performed at an earlier period; but as you are well acquainted with the labour required in the reduction of

so many observations by one person, it will appear to have been impracticable. I remain,

Dear Sir,

Your obliged and very obedient servant,

STEPHEN GROOMBRIDGE.

To the Rev. Dr. Maskelyne, Astronomer Royal, &c. &c.

WHEN I had fixed my four feet transit circle, made by TROUGHTON, it was my intention to make observations of a sufficient number of circumpolar stars, at different altitudes, to ascertain the latitude of my observatory with the greater precision; and also to endeavour to settle the true quantity of refraction: especially as my instrument was better adapted for that purpose, both from the construction and convenient size thereof, than those which had been heretofore made. Being fixed on stone piers, it is not so liable to partial expansion, as those instruments which are supported on brass frames: and having both sides divided, with two microscopes to each face; the same observation has the advantage of four made with a quadrant. I have also contrived, by means of sliding shutters in the roof, to prevent the rays of the sun from falling on any part, excepting on the object glass. By these means I flatter myself that, the observations having been made with great care and attention, the result will prove both satisfactory and useful.

Having proposed to myself the above course of observations, I selected the fifty stars contained in the first table for that purpose, the observations of which, exceed in number

1000. On the quantity of refraction to be assumed at 45°, astronomers not having been agreed, and Dr. MASKELYNE having suggested in the precepts to his folio tables, that Dr. BRADLEY having supposed the sun's parallax $10\frac{1}{3}''$, from which he inferred the refraction at 45° to be $57''$; if he had used the true parallax $8\frac{1}{4}''$, he would have found the refraction at the altitude of 45° to be $56\frac{1}{4}''$. I therefore assumed, and have constantly applied that quantity, correcting the same for the barometer and thermometer: the whole by the formula of Dr. BRADLEY.

In the above table, the third column is the mean of the observed zenith distance corrected by the equations to 1st Jan. 1807; the fourth is the mean of the computed refraction, for each observation; the sixth is the sum of the third and fourth colums, which gives the mean double zenith distance of the Pole, according to the assumed refraction in the seventh column; the eighth is the correction of the assumed refraction by the factor found ,02845, which is applied to the seventh column, and gives the true double zenith distance of the Pole in the ninth, the half of which is the corrected co-latitude in the tenth column. It appears that so far as η Ursæ majoris, the sum or difference of the zenith distances in the seventh column are sufficiently uniform to be used for the correction of the refraction. I then proceed to compare the first thirteen stars, where the zenith distance below the Pole is less than 60°, with the twenty-one following. From the former thirteen, the mean of the seventh column is 77°.3′ 53″, 0908; and the mean sum of the refractions in the fifth column is 94,″9377; from the latter twenty-one the means of the same columns are 77° 3′ 50,″5248 and 185,″1357. The difference

TABLE I. *Circumpolar Stars for the Refraction, reduced t*

Star.	No Observ.	Corrected Zenith distance.	Comp. Refraction.	Sum or difference.	Mean Zenith distance.	Sum or difference.	Cor. of Refraction.	Sum or diff. of true Zen. dis.	Co-Latitude.
		° ′ ″	′ ″	′ ″	° ′ ″	° ′ ″	″	° ′ ″	° ′ ″
Ursæ min.	6	37 35 10,59	0 44,66	1 29,63	37 35 55,25	77 3 52,67	2,55	77 3 55,22	38 31 57,61
Bode 4.	7	39 27 12,45	0 44,97		39 27 57,42				
Cephei.	16	37 14 4,10	0 42,93	1 31,88	37 14 47,03	77 3 52,30	2,61	77 3 54,91	38 31 57,45
Hev. 24.	8	39 48 16,32	0 48,95		39 49 5,27				
Polaris.	35	36 47 55,60	0 42,49	1 29,67	36 48 38,09	77 3 53,79	2,56	77 3 56,35	38 31 58,17
	41	40 14 28,52	0 47,18		40 15 15,70				
24 Ursæ	20	35 27 34,06	0 39,19	1 30,75	35 28 13,25	77 3 53,33	2,58	77 3 55,91	38 31 57,95
minor.	7	41 34 48,52	0 51,56		41 35 40,08				
ϑ Ursæ	27	35 5 21,71	0 38,78	1 31,20	35 6 0,49	77 3 53,13	2,59	77 3 55,72	38 31 57,86
minor.	5	41 57 0,22	0 52,42		41 57 52,64				
Camelop.	6	33 44 25,40	0 38,12	1 31,73	33 45 3,52	77 3 53,51	2,61	77 3 56,12	38 31 58,06
Hev. 29.	9	43 17 56,38	0 53,61		43 18 49,99				
α Cephei.	5	33 44 11,56	0 38,33	1 30,63	33 44 49,89	77 3 53,89	2,57	77 3 56,46	38 31 58,23
	7	43 18 11,70	0 52,30		43 19 4,00				
Camelop.	7	32 3 5,85	0 36,26	1 33,27	32 3 42,11	77 3 52,63	2,65	77 3 55,28	38 31 57,64
Hev. 30.	9	44 59 13,51	0 57,01		45 0 10,52				
ι Ursæ	14	30 51 25,00	0 33,00	1 33,48	30 51 58,00	77 3 53,21	2,67	77 3 55,88	38 31 57,94
minor.	10	46 10 54,73	1 0,48		46 11 55,21				
5 Ursæ	8	25 4 45,80	0 26,00	1 39,80	25 5 11,80	77 3 53,35	2,84	77 3 56,19	38 31 58,09
minor.	5	51 57 27,75	1 13,80		51 58 41,55				
β Ursæ	14	23 28 12,07	0 23,94	1 40,97	23 28 36,01	77 3 53,34	2,87	77 3 56,21	38 31 58,10
minor.	8	53 34 0,30	1 17,03		53 35 17,33				
γ 1 Ursæ	5	21 3 2,10	0 21,35	1 45,53	21 3 23,45	77 3 52,26	3,00	77 3 55,26	38 31 57,63
minor.	8	55 59 4,63	1 24,18		56 0 28 81				
γ 2 Ursæ	5	21 2 48,68	0 21,33	1 45,65	21 3 10 01	77 3 52,77	3,01	77 3 55,78	38 31 57,89
minor.	9	55 59 18,44	1 24,32		56 0 42,76				
ο Cephei.	30	15 35 5,97	0 15,70	1 58,57	15 35 21,67	77 3 52,67	3,37	77 3 56,04	38 31 58,02
	6	61 26 48,13	1 42,87		61 28 31,00				
α Dracon.	13	13 49 48,80	0 13,77	2 9,61	13 50 2 57	77 3 51,65	3,69	77 3 55,34	38 31 57,67
	6	63 11 53,24	2 55,84		63 13 49,08				
α Ursæ	13	11 19 11,17	0 11,15	2 15,51	11 19 22,32	77 3 51,82	3,86	77 3 55,68	38 31 57,84
major.	4	65 42 25,14	2 4,36		65 44 29,50				
ν Dracon.	26	10 29 0,80	0 10,27	2 21,45	10 29 11,07	77 3 52,36	4,02	77 3 56,38	38 31 58,19
	8	66 32 30,11	2 11,18		66 34 41,29				
ϑ Dracon.	33	7 36 53,00	0 7,38	2 37,23	7 37 0,38	77 3 51,32	4,48	77 3 55,80	38 31 57,90
	6	69 24 21,09	2 29,85		69 26 50,94				
2 Lyncis.	9	7 35 31,82	0 7,84	2 33,15	7 35 39,66	77 3 50,36	4,36	77 3 54,72	38 31 57,36
	7	69 25 45,39	2 25,31		69 28 10,70				
ϑ Ursæ	14	6 38 12,07	0 6,56	2 45,72	6 38 18,63	77 3 51,29	4,71	77 3 56,00	38 31 58,00
major.	5	70 22 53,50	2 39,16		70 25 32,66				
β Cassiop.	22	6 36 56,92	0 6,62	2 41,95	6 37 3,54	77 3 51,89	4,61	77 3 56,50	38 31 58,25
	9	70 24 13,02	2 35,33		70 26 48,35				
β Ursæ	29	5 56 42,74	0 5,84	2 49,68	5 56 48,58	77 3 51,32	4,83	77 3 56,13	38 31 58,07
major.	6	71 4 18,90	2 43,84		71 7 2,74				
ι Ursæ	26	5 32 27,53	0 5,42	2 54,81	5 32 32,95	77 3 51,29	4,97	77 3 56,26	38 31 58,13
major.	7	71 28 28,95	2 49,39		71 31 18,34				
ζ Ursæ	31	4 28 5,41	0 4,35	3 7,31	4 28 9,76	77 3 50,66	5,33	77 3 55,99	38 31 57,99
major.	7	72 32 37,94	3 2,96		72 35 40,90				
ν Persei.	10	3 36 58,63	0 3,65	3 7,75	3 37 2,28	77 3 50,67	5,34	77 3 56,01	38 31 58,00
	9	73 23 44,29	3 4,10		73 26 48,39				

-ade.	Star.	No. Observ.	Corrected Zenith distance.	Comp. Refract.	Sum or difference.	Mean Zenith distance.	Sum or difference.	Cor. of Refraction.	Sum or diff. of true Zen. dis.	Co-Latitude.
"			° ′ ″	′ ″	′ ″	° ′ ″	° ′ ″	″	° ′ ″	° ′ ″
57,61	γ Ursæ major.	20	3 17 59,02	0 3,23	3 15,31	3 18 2,25	77 3 50,17	5,56	77 3 55,73	38 31 57,86
		9	73 42 35,84	3 12,08		73 45 47,92				
57,45	ϑ Aurigæ.	9	2 47 3,31	0 2,84	3 13,12	2 47 6,15	77 3 48,36	5,49	77 3 53,85	38 31 56,92
		6	74 13 31,93	3 10,28		74 16 42,21				
58,17	9 Cassiop.	10	2 39 6,33	0 2,69	3 19,56	2 39 9,02	77 3 48,87	5,68	77 3 54,55	38 31 57,27
		7	74 21 22,98	3 16,87		74 24 39,85				
57,95	9 Bootis.	30	1 16 47,07	0 1,24	3 46,94	1 16 48,31	77 3 50,45	6,46	77 3 56,91	38 31 58,45
		4	75 43 16,44	3 45,70		75 47 2,14				
57,86	γ Persei.	7	1 16 20,81	0 1,25	3 36,98	1 16 22,06	77 3 49,63	6,17	77 3 55,80	38 31 57,90
		9	75 43 51,84	3 35,73		75 47 27,57				
58,06	9 Ursæ major.	14	1 4 52,51	0 1,08	3 43,16	1 4 53,59	77 3 48,85	6,35	77 3 55,20	38 31 57,60
		9	75 55 13,18	3 42,08		75 58 55,26				
58,23	β Dracon.	33	0 58 54,36	0 0,95	3 52,27	0 58 55,32	77 3 49,08	6,61	77 3 55,69	38 31 57,84
		7	76 1 2,45	3 51,31		76 4 51,76				
57,64	γ Dracon.	58	0 2 57,37	0 0,05	4 7,95	0 2 57,42	77 3 49,54	7,05	77 3 56,59	38 31 58,29
		8	76 56 44,22	4 7,90		77 0 52,12				
57,94	η Ursæ major.	57	1 11 11,67	0 1,16	4 29,82	1 11 12,83	77 3 48,77	7,94	77 3 56,71	38 31 58,35
		10	78 10 30,62	4 30,98		78 15 1,60				
58,09	α Persei.	23	2 18 14,89	0 2,30	4 44,60	2 18 17,19	77 3 48,71	8,09	77 3 56,80	38 31 58,40
		12	79 17 19,00	4 46,90		79 22 5,90				
58,10	9 Persei.	15	3 3 48,89	0 3,11	5 4,58	3 3 52,00	77 3 48,28	8,67	77 3 56,95	38 31 58,47
		8	80 2 32,59	5 7,69		80 7 40,28				
57,63	τ Herculis.	5	4 41 14,02	0 4,47	6 11,17	4 41 18,49	77 3 47,48	10,56	77 3 58,04	38 31 59,02
		5	81 38 50,33	6 15,64		81 45 5,97				
57,89	Capella.	24	5 40 45,74	0 5,63	6 42,24	5 40 51,37	77 3 47,71	11,44	77 3 59,15	38 31 59,57
		19	82 37 51,21	6 47,87		82 44 39,08				
58,02	ψ Ursæ major.	14	5 55 19,56	0 5,85	7 3,89	5 55 25,41	77 3 47,66	12,06	77 3 59,72	38 31 59,86
		5	82 52 3,33	7 9,74		82 59 13,07				
57,67	β Aurigæ.	9	6 33 13,88	0 6,50	7 27,85	6 33 20,38	77 3 46,77	12,74	77 3 59,51	38 31 59,75
		11	83 29 32,80	7 34,35		83 37 7,15				
57,84	α Cygni.	26	6 52 9,82	0 6,82	8 16,60	6 52 16,64	77 3 46,56	14,13	77 4 0,69	38 32 0,34
		6	83 47 39,78	8 23,42		83 56 3,20				
58,19	λ Ursæ major.	9	7 35 31,84	0 7,02	8 58,93	7 35 39,46	77 3 48,87	15,33	77 4 4,20	38 32 2,10
		5	84 30 21,78	8 6,55		84 39 28,33				
57,90	ι Aurigæ.	27	7 56 31,11	0 8,07	9 9,44	7 56 39,18	77 3 46,68	15,63	77 4 2,31	38 32 1,15
		8	84 51 8,35	9 17,51		85 0 25,86				
57,36	10 Ursæ major.	6	8 55 37,75	0 9,07	11 8,59	8 55 46,82	77 3 57,79	19,02	77 4 16,81	38 32 8,40
		5	85 48 26,95	11 17,66		85 59 44,61				
58,00	μ Ursæ major.	14	8 59 58,08	0 9,01	11 30,44	9 0 7,09	77 3 55,85	19,64	77 4 15,49	38 32 7,74
		5	85 52 23,49	11 39,45		86 4 2,94				
58,25	γ Androm.	12	10 4 1,34	0 10,20	13 40,83	10 4 11,54	77 4 1,01	23,35	77 4 24,36	38 32 12,18
		4	86 54 21,52	13 51,03		87 8 12,55				
58,07	β Bootis.	8	10 18 23,26	0 10,02	14 50,15	10 18 33,28	77 4 1,89	25,33	77 4 27,22	38 32 13,61
		2	87 7 35,00	15 0,17		87 22 35,17				
58,13	ε Aurigæ.	17	10 30 17,75	0 10,78	14 52,22	10 30 28,53	77 3 53,45	25,38	77 4 18,83	38 32 9,41
		4	87 19 18,98	15 3,00		87 34 21,98				
57,99	ζ Aurigæ.	9	10 41 4,42	0 10,91	15 47,19	10 41 15,33	77 3 50,09	26,94	77 4 17,03	38 32 8,51
		2	87 29 7,31	15 58,10		87 45 5,42				
58,00	β Persei.	11	11 15 42,06	0 11,29	18 13,44	11 15 53,35	77 3 54,68	31,11	77 4 25,79	38 32 12,89
		6	88 1 23,30	18 24,73		88 19 48,03				

of the polar distances, divided by the difference of the sum of the refractions, quotes ,0284485; which being increased by unity, is the factor to be multiplied into the assumed refraction, viz. $56\frac{1}{2}''$. Then the mean refraction at 45° will be $56,''5 \times 1,0284485 = 58,''10734$: the co-latitude

$$77° 3' 53,''0908 + \frac{94,9377 \times ,028845}{2} = 38° 31' 57,''897$$

and the latitude $= 51\ 28\ 2,103$,

which will be found the same, from the mean of the corrected co-latitudes in the tenth column, as given by the above thirty-four stars.

I have made these deductions from the fixed stars only; and therefore it will be required to compare the result with the sun at the solstices. I have preferred the former, from the greater number of observations to be so obtained, as well as from the more accurate bisection of a star, than the application of a wire to the limb of the sun; I have, however, endeavoured to render the observations of the sun, of equal consequence, by means of two fixed, and one moveable wire; the latter measures a space to one of the fixed wires, distant 29′ 45,″74 from the other, the sum of which is the diameter; the sun passing at the same time through the centre of the field. These wires are applied to the upper and lower limbs of the sun, by direct vision, the eye glass having a vertical, as well as a horizontal motion. The centre of the sun is found, knowing the distance of the extreme fixed wires, from that in the centre of the field; but the refraction must be applied for each limb, the difference of refraction at the winter solstice being 8″; therefore this diminution of the diameter must be noticed. The few opportunities I have had to observe the

solstices, have produced only eighteen observations at the summer, and thirteen at the winter, solstice. The proof resulting from this small number is, however, satisfactory; these being reduced to the same period, 1st January, 1807, are as follow:

	App. zen. dis.	Refraction.	Correct.	True zen. dis
sum. sols.	27° 59′ 38,″00	0′ 29,″83	+ 0″85	28° 0′ 8,″68
winter do.	74 52 24,16	3 26,84	5,52	74 55 56,02
				102 56 47,0
			Latitude	51 28 2,35

The mean obliquity of the ecliptic for the same period will be

	74° 55′	56,″02
	28 0	8,68
	46 55	47,34
	23 27	53,67

The new solar tables state the obliquity of the ecliptic for 1800 at 23° 27′ 57″, and taking the decrease at half a second per annum, shews the above result to be very near the truth.

I shall now compare the observations of Lieut. Col. MUDGE, made at the Royal Observatory in 1802, with his zenith sector, to ascertain the difference of latitude between that place and my observatory: the following stars being reduced to the same epoch $\beta\gamma$ 45, 46, 51, Draconis; $\varkappa$, ι, Cygni; γ, π, Ursæ majoris; ι, Herculis, and Capella. The mean difference of the zenith distances of these stars, and my own observations, is 35,″49; which being added to my latitude, shews the latitude of the Royal Observatory to be 51° 28′ 37,″59;

and this subtense of the arc agrees very nearly with that of γ Draconis, by Dr. MASKELYNE, which he has communicated to me, being 35,″31. Several of those made by Lieut. Col. MUDGE were single observations, with the zenith sector in one position only, and therefore were reduced by the mean collimation, found from the other stars; but the result shews the accuracy of his observations. The latitude of Greenwich, when corrected from the mean of circular instruments, as proposed by Mr. POND, and by the refraction for the excess of 57″, will not differ more than half a second, from the above determination.

Having found by this hypothesis, the mean astronomical refraction at 45°, I shall proceed to compare the same, with the observations of other astronomers. M. PIAZZI has very ingeniously, and with great labour, made his deduction of the mean refraction, from the observations of Procyon, α Lyræ and Aldebaran at equal altitudes, from 38° to 89° 30′ zenith distance, in addition to several circumpolar stars, for his latitude: this advantage having been obtained from his instrument moving in azimuth. The result thereof gives the mean refraction at 45° = 57,″3 into the formula of Dr. BRADLEY, tang. $\overline{z-3r}$; which I have found, as above, 58,″107. M. PIAZZI, therefore, shews in his table of mean refractions, that at 45° = 57,″3 × tang. $\overline{45°-3r}$ = 57,″2; which will be = 58″,01 in my table. On a comparison with the present French tables, the latter give 58,″2, at 45°, which is in excess of my own. The formula of M. DE LA PLACE, as given in his Mecanique Celeste, which is very elaborate and ingenious, being applied to a constant quantity, 60,″616, as determined from obser-

vation, will produce a table of refractions, nearly the same as Dr. BRADLEY's, though the latter is effected by a more simple formula. M. PIAZZI has shewn from his observations, that the actual refractions, so far as 80° from the zenith, are greater than those assumed by Dr. BRADLEY; and thence to the horizon, the formula of the latter produces an excess, therefore requires a reduction in the quantity. The ninth and tenth columns of the above fifty stars, will show the same result as that of M. PIAZZI; viz. that below 80° the assumed refraction is greater than will be given from observation: and the mean correction required for the four last stars, from 87° to 89°, will be — 0,027, which he determines — 0,039. Those stars from the zenith to 80°, I have already corrected, by assuming a greater constant quantity; whence it follows, that all those equations will be plus, as compared with Dr. BRADLEY; but this will not happen with those at lower altitudes, since his formula thence fails: and it is the correction thereof, which I shall next proceed to investigate.

This problem of the mean refractions, so important to practical astronomy, which has occupied the attention of many mathematicians, has always had in view, to reconcile the known laws of the refraction of a ray of light passing through different media, with the actual quantity deduced from observation. The investigations of various authors are given in the first volume of Mr. VINCE's Astronomy; and that of Mr. SIMPSON, in his Mathematical Dissertations. It has been proved, that the refractions vary nearly as the tangents of zenith distance, into a constant quantity; and assuming the variation directly as the tangents, the approximation will not be sensibly affected so far as 60°; but comparing these, with those at lower alti-

tudes, as found from observation, it will appear that the refractions vary, as the tangents of zenith distance, minus some multiple of r (the refraction), which call y. Now the value of y will be found by the formula drawn from the investigation given at large in Mr. VINCE's Astronomy, by comparing the observed refraction, at different zenith distances, putting r = refraction, z = zenith distance of the highest star; r' and z' of the lowest star: then

$$y = \frac{\overline{r \times \text{cot. } z - r' \times \text{cot. } z'}}{r^2 - r'^2}.$$

Applying this formula to the mean refraction of Polaris below the pole = 49″,105, compared with the corrected or observed refraction of each of the last three stars; the mean value of y = 3,3625. Dr. BRADLEY found by comparing the refraction at 60° with that at 90°, which he supposed 33′ 0″, the value of y = 2,996; he therefore assumed 3. M. CASSINI found the same to be 3,226; M. BOUGUER 3,393; and Mr. SIMPSON 2,75: this difference arises from their having supposed the horizontal refraction greater than it appears to be from observation. In the dissertation of the last author, he proves, that above 7° altitude, it matters not whether you assume the refraction, according as it would be found from the increased density of the air, at the low altitudes, and which would give the refraction at 90° about 52′, or by an uniform density, which agrees better with observation; since, in the former case, it would only affect the refraction about two seconds.

Having thus assumed a greater value of y, than 3, for the coefficient of r, the mean refraction at 45° will vary inversely, as tang. $\overline{z - 3r}$: 58″,10734 :: tang. $\overline{z - yr}$: x, therefore,

$$x = \frac{\text{tang. } 45° - 3r \times 58,10734}{\text{tang. } 45° - yr} = 58'',1192.$$

The table of mean refractions by the proposed formula, compared with the observed refraction, differs with the lowest star, less than 2"; but the refractions at those low altitudes having always been found uncertain, we may presume that the result so nearly approximates the truth, that we may not possibly ascertain the refractions more correctly. However, since my deduction will soon be submitted to the test of some excellent circular instruments, it will be seen, whether more accurate tables can be formed.

M. BIOT, in the report of the National Institute, observes, that a third correction of the refractions having been supposed to arise from the vapours at low altitudes, and therefore an equation might be found from the hygrometer, he was induced to make some experiments with a prism containing warm air dried by potash, the outside of which was charged with the natural moisture of the atmosphere; but he could discover no alteration in the refraction. He consequently infers, that the hygrometrical state of the atmosphere has no sensible effect on the refractions. Supposing an effect really to exist, a standard hygrometer would be required; since those instruments, which show the comparative state of humidity or siccity of the atmosphere, give no scale of the velocity of the current of air which causes the change.

The mean astronomical refraction being determined in this manner, it will be further necessary to investigate the equations for the barometer and thermometer; and I must premise, that I have always applied those for the latter, from the temperature shewn by the thermometer attached to the telescope. Some astronomers have applied the correction for the external temperature, others for the mean of both; but I believe

the majority have adopted the temperature within the observatory.

It is to be regretted, that astronomers have not informed us, by what methods they have deduced these corrections of the mean refractions; I shall therefore explain that which I have adopted, in order that this hypothesis may be investigated; and any error therein, be the more easily detected. I have taken all those stars in the table, below the Pole, from β Ursæ majoris to ε Aurigæ, both inclusive, having noticed that those observations made in winter, when the correction is additive, the assumed correction by Dr. BRADLEY's formula appears to be too great; and this difference will be seen by the comparison in Tables II. and III. To these I have added Fomalhaut, containing in the whole 210 observations. Having determined the mean place of the star at a certain epoch, I find the apparent place; the difference between which and the observed place, is the apparent refraction, which call a; put b = mean refraction, c = to the correction for the barometer, and w = to the correction for the thermometer: then $\frac{a-b}{b} - c = w$.

The following observation of α Cygni will suffice for an example. Observed zenith distance 83° 47′ 58″,34; apparent zenith distance 83° 56′ 27″,12; apparent refraction 8′ 28″,78; mean refraction 8′ 7″,83; barometer 30,13; correction by BRADLEY +,0179; thermometer within 41, without 34: then $\frac{a\,(8'.28'',78) - b\,(8'.7'',83)}{b\,(8'.7'',83)} = +,0430 - c\,(+,0179) = w\,(+,0251)$ for the thermometer within at 41, or without at 34.

Proceeding in this manner with the above 210 observations, I obtained equations for the thermometer within, from 31 to

79; and for the thermometer without, from 94 to 80. The difference of these extremes being divided by 8, will show the corrections for the former to every 6°, and for the latter to every 7°; and assuming that the variations are in a reciprocal ratio to the degrees of the thermometer, I propose the following formula. Putting h for the degree of FAHRENHEIT's thermometer, then $\overline{49^\circ - h^\circ} \times ,0024$, when below the mean; $\overline{49^\circ - h^\circ} \times ,0023$, when above the mean; will be the correction for the thermometer within; and $\overline{45^\circ - h^\circ} \times ,0021$, the correction for the thermometer without. The Tables II. and III. show the near agreement of the observed and the equated factor, found as above. The barometer at its mean state may continue to be taken at 29,6 inches; and the mean point of the thermometer without, will then be at 45°. The thermometer within, which has been assumed at 50° for the mean state, from observation, appears to be at 49°; and supposing the barometer and thermometer to vary reciprocally, as the former in inches and the latter in degrees, the mean point of each is in an inverse ratio to the other, and might be changed if thought expedient. Indeed it appears from the mean of more than 1000 observations of these circumpolar stars, at all seasons, that the mean state of the barometer is 29,85, and of the thermometer 52°; yet, as the sum of the two corrections would still be the same, there cannot be occasion to alter the mean point of the former, especially as this might not be a general rule for every climate. It has very rarely happened, that I could make any observation, when the barometer has been below 29 inches, which causes the mean state to be so high as 29,85; but it may be otherwise in places,

where the atmosphere is not so charged with vapours, as that of this country.

I have, in the Table IV. shewn the mean refractions, according to various authors; the first column contains those of Mr. SIMPSON, from the observations of Dr. BEVIS; the second, those of Dr. BRADLEY, by his formula, tang. $z - 3' \times 57''$; but this quantity having been found, from later observations, to have been too small; the third column contains the same formula, into $58'',107$; yet the refractions, at low altitudes, will be found too great, and this excess has been corrected by M. PIAZZI, using arbitrary equations, which will bring them nearer to the truth, as shown in the fourth column. The more uniform correction of the whole will be, as I have proposed in the above theorem, by increasing the coefficient of r; a corollary from which will be drawn, that the increment of the tangents near the horizon will cause the equations of M. PIAZZI to vanish. The fifth column contains the refractions of the French tables; and the sixth, my own; viz. tang. $z - 3.3625r \times 58'',1192$.

On inspection of this table, it will appear, that the refractions by different formula do not vary considerably, so far as 80°; and thence to 87°, my deductions are rather in excess of the French tables; below which, mine are rather less. Whether this arises from the defect of the formula, I cannot determine; since the wall of Greenwich park being above my horizon, I cannot observe lower than 88½°: however, so far as 88°, the formula I have proposed, agrees with observation.

S. GROOMBRIDGE.

Blackheath, 22 Jan. 1810.

Factors for the correction of the Refraction, the mean result of 210 observations.

Thermometer within.				Thermometer without.		
	Bradley.	Observed.	Equated.		Observed.	Equated.
31	+,0499	+,0437	+,0436	24	+,0436	+,0436
37	,0336	,0329	,0295	31	,0319	,0289
43	,0178	,0155	,0155	38	,0153	,0142
49	,0025	,0000	,0014	45	,0007	—,0005
55	—,0125	—,0132	—,0126	52	—,0161	,0152
61	,0275	,0286	,0267	59	,0291	,0299
67	,0425	,0444	,0407	66	,0480	,0446
73	,0575	,0512	,0548	73	,0587	,0593
79	,0725	,0688	,0688	80	,0741	,0740

Factors for the correction of the Refraction, equated from the mean of the above, according to the proposed formula.

Table II.

Thermometer within.	
28	+,0504
32	,0408
36	,0312
40	,0216
44	,0120
48	,0024
49	,0000
52	—,0069
56	,0161
60	,0253
64	,0345
68	,0437
72	,0529
76	,0621
80	,0713

Table III.

Thermometer without.	
20	+,0525
25	,0420
30	,0315
35	,0210
40	,0105
45	,0000
50	—,0105
55	,0210
60	,0315
65	,0420
70	,0525
75	,0630
80	,0735
85	,0840

Table IV. The mean astronomical Refractions, according to several Authors.

Zenith dis.	Simpson.	Bradley. tang. $\frac{z - 3r}{s}$ × 57″	Bradley. tang. $\frac{z - 3r}{s}$ × 58″,107.	Piazzi	French Tables.	S. G.
°	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
10	0 9	0 10,00	0 10,19	0 10,2	0 10,3	0 10,24
20	0 19	0 20,70	0 21,10	0 20,8	0 21,2	0 21,13
30	0 30	0 32,90	0 33,54	0 33,2	0 33,4	0 33,51
40	0 44	0 47,80	0 48,73	0 48,1	0 48,9	0 48,69
45	0 52	0 56,90	0 58,00	0 57,2	0 58,2	0 58,01
50	1 2	1 7,80	1 9,11	1 8,2	1 9,3	1 9,11
55	1 14,5	1 21,20	1 22,77	1 22,4	1 23,1	1 22,77
60	1 30	1 38,40	1 40,31	1 39,8	1 40,6	1 40,29
65	1 52	2 1,70	2 4,02	2 3,5	2 4,3	2 3,98
70	2 23	2 35,50	2 38,50	2 37,8	2 38,8	2 38,41
72	2 40	2 53,90	2 57,28	2 56,5	2 57,6	2 57,13
74	3 1	3 16,70	3 20,44	3 18,3	3 20,6	3 20,22
76	3 27	3 45,50	3 49,79	3 47,3	3 49,8	3 49,44
78	4 2	4 23,18	4 28,23	4 24,3	4 27,9	4 27,68
80	4 50	5 14,83	5 20,78	5 16,1	5 19,8	5 19,85
81	5 21	5 48,45	5 55,00	5 47,4	5 53,5	5 53,74
82	5 59	6 29,55	6 36,85	6 28,3	6 34,4	6 35,06
83	6 48	7 20,84	7 28,98	7 19,5	7 24,7	7 26,46
84	7 49	8 26,41	8 35,61	8 24,9	8 29,9	8 31,85
85	9 10	9 52,50	10 3,18	9 45,4	9 54,3	9 57,27
86	11 5	11 49,77	12 1,95	11 42,6	11 48,3	11 52,21
87	13 44	14 34,61	14 48,78	14 25,1	14 28,1	14 31,75
88	17 43	18 34,80	18 50,59	18 2,7	18 22,2	18 19,19
89	23 50	24 28,14	24 46,42	23 46,1	24 21,2	23 46,77
90	33 0	32 59,43	33 18,52	32 3,0	33 46,3	31 27,87

XI. *Extract of a Letter from the Rev.* John Brinkley, *D. D. F. R. S.* Andrew's *Professor of Astronomy in the University of Dublin, to the Rev.* Nevil Maskelyne, *D. D. F. R. S. Astronomer Royal, on the annual Parallax of α Lyræ.*

Read April 12, 1810.

I HAVE now had sufficient experience of my eight feet circle, to be highly satisfied with it, and have arrived at one conclusion, that it is of importance in astronomy.

My observations on α Lyræ for the purpose of discovering an annual parallax now amount to 47 in number, viz. 22 near opposition, and 25 near conjunction, and the mean of these gives a result of 2,″52 as the parallax of the annual orbit for that star, and I have no doubt that it exceeds 2″.

My observations of different circumpolar stars, and of the same star in different states of the thermometer, seem to require a small alteration in the numbers of Dr. BRADLEY's formula for refraction.

The formula so altered is

$$\text{Refraction} = 56'.9 \times \text{tang.} \left\{ \text{Zen. dis.} - 3.8\ \text{Refr.} \right\} \times \frac{\text{height of barom.}}{29.6} \times \frac{500}{450 + \text{ther.}}$$

By means of this formula, the observations of circumpolar stars considerably distant, give the same co-latitude to a great degree of exactness.

XII. *On the Mode of breeding of the Oviviviparous Shark, and on the Aeration of the fœtal Blood in different Classes of Animals.* *By* Everard Home, *Esq. F. R. S.*

Read June 7, 1810.

THAT some of the shark tribe do not lay their eggs, but hatch them within the body, and that others lay them in the same manner as the skate, has long been known; but nothing seems to have been accurately made out upon either of these subjects, I am therefore induced to lay before the Society the following observations.

In my examination of the squalus maximus, an account of which is published, I found that it closely resembled in its internal structure, the squalus acanthius of LINNÆUS, a fish common on the Sussex coast, which made me pay particular attention to the anatomy of all the parts of this species of dog-fish.

In December, which is the breeding season of this particular species of dog-fish, I procured specimens of the male, and of the female, in all the different stages of impregnation; from the dissection of these is drawn up the following account of the organs of generation.

The male organs were found in two very different states; in one, the testicles were small, and the penis scarcely discernible; in the other, the testicles were larger, the epididymus and vas deferens turgid with semen, and the penis put on the appearance of a projecting infundibular canal, capable of con-

veying the semen into the oviduct of the female, resembling in its form, the penis of the squalus maximus, only upon an infinitely smaller scale.

The pendulous bodies close to the oval fins of the male, which have been mistaken for penises, by many physiologists, and by others more rightly considered as claspers, to lay hold of the female, bear a close resemblance in their form to those of the squalus maximus, which have been described in my account of that fish, under the name holders, and from every part of their mechanism, there can be no doubt that they perform that office. In the breeding season, they become more fleshy than at other times, the muscles at that time being enlarged.

The insertion of the penis into the female, is not unlike that of the common fowl, but the penis is fitted to inject the semen further into the oviduct than can be done by the grooved penis of the cock.

When the holders are spread out, so as to embrace the female, even after the fishes are dead, the penis is brought forward by that means, so as readily to enter the external orifice of the oviducts, to which it is guided by the prominence of the clitoris. The holders are represented in this expanded state in the annexed drawing.

The mode in which the semen enters the penis is so unlike what is met with in other animals, that it deserves to be particularly explained.

The vasa deferentia are convoluted in their course, but become straight and much enlarged at the lower part, and instead of going on to the penis, terminate by two wide orifices on the posterior surface of what may be called the

urinary bladder, which is of an oval shape, and partially divided into two by a septum, on each side of which the ureters enter it. From this cavity, the penis is continued like the neck of a Florence flask, and the semen, before it can arrive at the penis, fills the bladder, and is propelled by the action of the muscular coats of that cavity, by which means the semen acquires velocity, and the penis is rendered turgid during the whole time that such force is applied to the liquid passing through it.

This bladder at other times is a reservoir for the urine, which must be considered a secondary office in a fish constantly living in water.

The situation and appearance of the testicles are seen in the annexed drawing, which makes a description of them unnecessary.

The female organs, before they undergo the necessary changes to form the eggs, and prepare them to receive the influence of the male, are very little developed; the ovaria are not larger than the testicles of the male before the breeding season, and resemble them both in their appearance and situation; the oviducts are so small as with great difficulty to be traced, and the clitoris is just large enough to be distinguished. When the eggs are formed, all these parts develope themselves.

The ovaria became exceedingly vascular, and the yolks become conspicuous in all the stages of their increase, from the size of a pea to that of a walnut, and when arrived at that size, they pass into the oviduct.

The number of yolks which are ready at any one time to pass into the oviduct, varies exceedingly in different fishes,

and even in the two ovaria of the same fish. I have seen five of the full size in one ovarium, and only two in the other; in another fish there were three in each, and so on to a great variety.

The oviducts enlarge, and become exceedingly extended. In a fish 27 inches long, each oviduct was 26 inches in length; its internal surface was formed into three distinct cavities, separated from one another by contractions in the coats of the canal. The first of these begins from the orifice that receives the yolk, and is pyramidal in its form; it is ten inches in length, and gradually diminishes in its capacity, the coats being extremely elastic, which throws the internal membrane into folds in a longitudinal direction, taking a very serpentine course. At the termination of this portion, the contraction is formed by the interposition of a substance of a fibrous structure, and light grey colour, between the external coat and internal membrane, forming a circular band, which is divided into three equal parts by two circular parallel lines on the internal membrane. This band is half an inch broad, and its internal surface has a glandular appearance. The second cavity is only six inches long, its internal membrane is very vascular, and thrown into plicæ in a longitudinal direction, longer, thicker, and less numerous than those of the first portion; they are smeered over with mucus. This cavity is separated from the third, by a transverse fold of the internal membrane in a contracted state.

The third portion is ten inches long, and forms the cavity in which the eggs are retained, till the young fishes are formed, and capable of taking care of themselves. The eggs, however, are not loose in the oviduct, as in birds, but a certain number,

corresponding to the number of yolks that are in a state to leave the ovarium at the same time, are inclosed in a membranous bag, piled one upon the other. This bag, at its upper end, is grasped by the contraction which separates the middle and lower portion of the oviducts; the other extremity, which is in the form of a blunted cone, is loose and moveable in the surrounding cavity; the eggs are inveloped in a transparent jelly, which occupies every part of the bag, beyond that in which the eggs are contained.

The clitoris becomes so much enlarged as to project externally; its base swells out into the form of a heart, as it is painted upon cards, only that it is much more pointed; the posterior surface adheres closely to the parts behind it, for one half of its length, the other half is loose, and on its upper surface there is a groove passing on each side, towards the orifices of the two oviducts. The nymphæ are formed by a fold of the termination of the rectum, and project laterally; they are very vascular, and compose the external orifice of the vestibulum, beyond which are the contracted openings of the oviducts. After impregnation has taken place, the pendulous portion of the clitoris becomes flaccid and narrow.

When the young dog-fish is completely formed, the yolk remains attached to the belly by a long chord, consisting of blood vessels, and the fish swims about in the surrounding jelly with this attached to it. If the bag is torn, and the fish is taken out and put into water, it swims about, but if the vessels going to the yolk are wounded, the fish immediately dies.

What number of the shark tribe have this particular mode of hatching their eggs is not at present known, but there is

reason to believe, that it is very general, since the eggs or the empty shells belonging to this tribe of fishes, are rarely met with.

The squalus maximus, I have no doubt, is of this kind; and from the following memorandum of my late friend Dr. PATRICK RUSSELL, given to me by Sir JOSEPH BANKS, the large shark met with between the Tropics, breeds in the same way.

"A shark caught on the 14th of November, 1781, in lat. 7° N. measured 8 feet 7 inches, the tail included. The head and body of a dark bluish colour, the breast and belly of a silver white. Upon opening the belly, the two oviducts were distended with young ones contained within an inner cavity, swimming in a white gelatinous liquid, thicker than the liquor amnios of quadrupeds.

"The right oviduct contained 21 young ones, the left 20; of these 25 were males, and 16 females; they were all nearly of the same length, between 9 and 10 inches, and each of them had the yolk attached to its belly, by a chord of considerable length.

"In the lat. 5° N. another female shark was caught 7 feet long; it had only 8 young ones, 4 on each side."

Of the oviparous shark I could obtain no information, but what is contained in BOHADSCH, in whose works there is an engraving of the egg, and of the shark to which it belonged, caught in the Mediterranean. So little has this subject been attended to by naturalists, that no mention is made that I know of, in any author, of such eggs being met with upon the coast of Great Britain; I was, however, so fortunate as to find a shark's egg on the sea beach at Worthing in Sussex, in Sept. 1809, and in the course of that month, procured several

of them containing young sharks in all the different stages of their growth. The egg which is represented in the annexed drawing, is exactly similar to the engraving in Bohadsch: when minutely examined, there is on each side of the egg a small slit, for the admission of salt water within the shell.

In the latter end of October, a young dog-fish was sent me 13 inches long, which exactly resembled the embryo in the egg, and proves to be the squalus canicula of Linnæus; it had been feeding on the worms met with in the sand banks, some of which were found in its stomach.

The oviviparous and oviparous dog-fishes, differ materially in the form of their stomachs. In the first, the pyloric portion is short and wide, in the other, long and narrow like an intestine. As there are larger sharks with both these kinds of stomach, it is reasonable to believe they have also the same difference in their mode of breeding.

The gelatinous liquid which surrounds the ova of the dog-fish, was found to differ exceedingly in its properties from other animal jellies, for when the membranous bag containing it, was immersed in proof spirit, for the purpose of preserving the ova in their natural situation, the jelly, instead of coagulating, as was expected, expanded so much as to burst the bag. So striking a peculiarity led to a further consideration of it, and a suggestion naturally arose in my mind, that there might be a similarity between it and the jelly, with which the ova of the frog are surrounded. I therefore requested my friend Mr. W. Brande, to examine the chemical properties of both these substances, and shall subjoin his account of them.

That the jelly in the oviviparous dog-fish, is formed in the

middle portion of the oviduct, there can be no doubt; but the exact part in which the jelly of the frog is formed, has not been ascertained, which led me to make the following observations respecting it.

In some frogs that were kept in a damp cellar through the winter, no visible change took place in the ovaria or oviducts, although frequently examined, by immersing parts of them in water at different temperatures, till the 10th of February; at that time a portion of the oviduct, when immersed in water at 80°, swelled out so as to double its size, and when the water was at 120° expanded very rapidly. On the 25th of February the oviducts were seen to enlarge, and on slitting them open, the internal surface was smeared over with a ropy fluid. A portion of oviduct two or three inches long, in water heated to 120°, swelled out into a mass of transparent jelly which filled a half pint tumbler, and all traces of the coats of the oviduct were lost.

Upon shewing this jelly to Sir JOSEPH BANKS, he said that it very much resembled what he had seen when a boy, in the country during the winter months, upon the ground, and on the boughs of trees, called star-shot jelly, from being supposed to be formed by falling stars; and that it would be worth while to compare them together, and determine whether the common opinion mentioned by PENNANT, which Sir JOSEPH BANKS had always believed, was correct. The jelly is said to be brought into this state, by the frog having been swallowed by a bird, and the warmth and moisture of the stomach, making the jelly in the oviducts expand so much, that the bird is obliged to reject it by vomiting. Sir JOSEPH BANKS procured some star-shot jelly from Lincolnshire, and Mr. W. BRANDE

found that it was in all its chemical properties, the same with that formed in the oviduct of the frog, and when saturated with water, resembled the jelly in the oviduct of the dog-fish.

The consideration of the properties of this jelly, with which the ova of the oviviviparous dog-fish and of the frog, are surrounded, and the purposes it is intended to answer, led me to take a view of the various modes by which the fœtal blood is aerated in the different classes of animals. The provisions of nature for this very important purpose, form a beautiful series.

The ova of many fish, as the salmon and trout, are laid in the sand and gravel, and the fœtal blood of the embryo is aerated by means of the surrounding water; these fishes, therefore, are found to spawn as near as possible to the sources of springs, where the water, coming from the earth, is in a state of high aeration. The ova of other fish, which spawn in waters less impregnated with air, are deposited on the leaves of water plants and weeds, that give out oxygen, by means of which, the embryo has its blood aerated. The tench and the pike are of these kinds.

The ova of the perch are surrounded by a jelly as that in the frog, which imbibes water, probably for aerating the fœtal blood.

These observations were suggested to me, by my friend Professor DAVY, who has made it a part of his amusements to attend to the habits of fishes.

The ova of the oviparous shark, skate, and all that tribe, although too strong in their coats to be penetrated by the sea water, have natural apertures for its admission and escape,

there being two on each side of the egg, so that there is a current of sea water constantly supplying the egg with air.

In the ovivivíparous sharks the ova have no hard covering, being contained within the body of the fish, they are surrounded by the same jelly as in the frog, and the sea water is applied to the membrane in which they are inclosed, the opening of the oviduct admitting it for that purpose.

In the bird, the eggs being surrounded by atmospherical air, are supplied with it through the porous texture of the shell, which readily transmits it to the membranes of the embryo.

In all these tribes, the mode, in which the air is applied to the fœtal blood is as follows. Besides the common circulation of the blood from the heart of the embryo, to the different parts of its frame, there is a lesser circulation from the arteries to the membranes, which inclose the embryo, and which are in contact with the aerated water, or air, in which the egg is deposited; in this circulation, the blood attracts the air through the membrane, and conveys it into the system. In birds, the young are fed and taken care of by the parents, after the eggs are hatched, and in them this fœtal circulation is immediately stopped, on the embryo breaking the shell, and the yolk is drawn up into the belly as a supply of nourishment, till the stomach has acquired the powers of digestion.

In the ovivíparous sharks, the young ones swim about in the jelly, which surrounds the ova, with the yolk attached to the belly by a long vascular chord, till fitted to swim in the sea, and all this time the blood is aerated from these membranes. After the young leaves the bag in which the eggs are contained, the yolk is taken into the belly, as in the bird.

In the opossum tribe, and all the pouched animals in New South Wales, there is a peculiarity in the form of the female organs of generation, which has not been understood; this is a communication between the uterus and vagina during the formation of the embryo, by means of one or two lateral canals. These I described in my account of the female organs of generation of the Kanguroo, and was led to believe, from the coagulated state of the parts, which had been a year preserved in spirit, that the openings into the vagina were closed; but this mistake was afterwards corrected in my account of the Wombat, in which I stated from Mr. BELL, who examined the uterus with the embryo in it, "On opening the vagina, it was found to terminate at the common neck of the uteri; on each side of which, were the openings of the lateral canals, and in the middle between them, the meatus urinarius, with a slender fleshy pedicle on each side of it. Behind the meatus urinarius, were two orifices leading to the uteri, but they were filled with a thick gelatinous substance, which rendered them completely impervious." These lateral canals enlarge very much, as the embryo begins to form; at one end they open freely into the uterus, and at the other into the vagina; they are distended with jelly, and are employed to keep up a communication with the external air, for the purpose of aerating the fœtal blood; their appearance in the kanguroo is seen in the annexed drawing.

As the embryo in all the animals of this tribe is contained in a soft egg, which, from the description of Mr. BELL, the only person who has attentively examined it, has no connection with the internal membrane of the uterus, some apparatus like the lateral canals became necessary for the aeration of

the fœtal blood. The embryo in these animals, therefore, forms the intermediate link between the ova that are deposited in the oviduct, and hatched there, and the fœtus formed in the uterus; it resembles the first, in the mode by which the fœtal blood is aerated, and the second, in the situation in which it is deposited, and in the mode of its being supplied with nourishment after birth.

In quadrupeds in general, and the human species, the fœtal blood is aerated in a very different manner, from that which has been described; for although the fœtal circulation sends a portion of its blood to the whole of the membranes in which the embryo is inclosed, as in oviparous animals, the influence of the external air is excluded, from the coats of the uterus, to which these membranes every where adhere. The fœtal blood is however close enough to that in the vessels of the uterus, to have the air in the maternal blood communicated to it.

There are three different structures of the membranes of the fœtus for this purpose; one of these is the whole of the fœtal membrane next the uterus, being extremely vascular, as in the horse and ass; the second, is having only particular portions of it loaded with blood vessels, as in the deer; the third, is having only one large mass, into which the fœtal blood vessels pass to a certain depth, beyond which is a cellular structure, filled with blood from the arteries of the uterus, and taken up by the uterine veins, so that in this instance, there is a greater mass of blood belonging to the mother, nearly in contact with the fœtal blood, than in other animals.

From the series of facts which has been detailed, it appears, that oxygen is essentially necessary for the action of the

vivifying principle in all classes of animals; and that it is not simply applied to the blood, but mixed with that fluid while circulating in the arteries, since, if this were not the case, there would be no means of aerating the fœtal blood in the higher orders of animals.

Experiments on the Jelly formed in the Oviduct of the Frog, and Oviviviparous Shark, by Mr. W. Brande, *F.R.S.*

The substance contained in the oviduct of the frog, is of a whitish colour, nearly opaque and extremely viscid. It does not mix with water, but at common temperatures expands slowly in that fluid to about twelve times its original bulk.

At a temperature of 100°, this expansion is much more considerable, and the substance puts on the appearance of an attenuated and nearly transparent jelly, but none of it is dissolved.

The expansion seems to depend on the absorption of water by the gelatinous substance, the proportion of water taken up being very great. A piece of the unexpanded substance of the size of a large pea, requires rather less than three ounces of water for its complete expansion, and a mass of jelly equal in bulk to three ounces is formed.

This substance dried at a temperature of 212°, becomes brittle, but when it is put in this state, into moderately warm water, it again expands, becoming nearly as bulky as before.

Digested in alcohol it becomes brittle and opaque, and contracts to about one half of its original bulk. If in this contracted and perfectly brittle state, it be put into warm water, it again expands as before. But when once expanded, it is neither hardened nor coagulated by alcohol.

It is soluble in nitric, sulphuric, and muriatic acids.

Nitric acid, diluted with its weight of water, when poured upon the substance recently removed from the oviduct, changes its colour to a deep yellow, and rapidly dissolves it on the application of a moderate heat. This solution is of a pale yellow colour. The caustic fixed alkalies render it slightly turbid when not added in excess; in this case the mixture becomes perfectly transparent.

Muriatic acid at a boiling temperature, dissolves the recent substance very rapidly, forming a solution of a deep blue colour,* in which no precipitation is produced by the alkalies.

Concentrated sulphuric acid dissolves the substance from the oviduct slowly, and forms a pale brown solution. If heat be applied, the colour approaches to black.

No change is produced by the alkalies in these sulphuric solutions.

The substance is very rapidly dissolved by a boiling solution of caustic potash. The compound is imperfectly saponaceous, its transparency is not disturbed by the addition of sulphuric or of muriatic acid; but nitric acid added in a small excess, renders it slightly turbid.

None of the solutions which have been described, afford any precipitation on the addition of tannin, neither does water, in which the substance has been boiled, yield even the smallest traces of gelatine.

* The blue colour of this solution, is instantly destroyed by the addition of an alkali: it seems to arise from the formation of a very minute portion of prussiate of iron. Mr. HATCHETT has observed, that some of the varieties of albumen afford a blue solution, when long digested in muriatic acid: this is probably from the same cause. Phil. Trans. 1800.

No coagulation is produced in this substance, by VOLTAIC electricity, from 30 double plates of four inches.

These experiments shew, that the substance from the oviduct of the frog, is of a peculiar nature: its characteristic property being the remarkable power of expansion, by the absorption of water.

It is distinguished from gelatine by its insolubility in water, and by affording no precipitate with solutions containing tannin: from albumen, by not coagulating on the application of acid, or electricity, and by forming compounds with the alkalies, which are not saponaceous. In some of its other properties, it would appear, as far as regards its chemical habitudes, to be a substance, intermediate between albumen and gelatine.

The gelatinous substance from the egg of the dog-fish, has the properties of that from the oviduct of the frog in its expanded state, and is consequently analogous to what has been termed star-shot jelly.

EXPLANATION OF THE PLATES.

PLATE IX.

An internal view of the belly of the oviviviparous dog-fish, squalus acanthias, of LINNÆUS: to show the organs of generation in their natural situation, when in a state fit for impregnating the female.

a The heart.

b b The liver.—The left lobe cut away to expose the parts behind.

c The æsophagus.

d The cardiac portion of the stomach.

e The pyloric portion.

f The small cavity between the pylorus and duodenum.

g The duodenum.

h h The valvular intestine.

i i An appendix to the intestine with which it communicates.

k k The testicle.

l l The vas deferens, the lower portion of which is straight, and distended with semen.

m The cavity which is the reservoir of semen, and urinary bladder.

n The kidney.

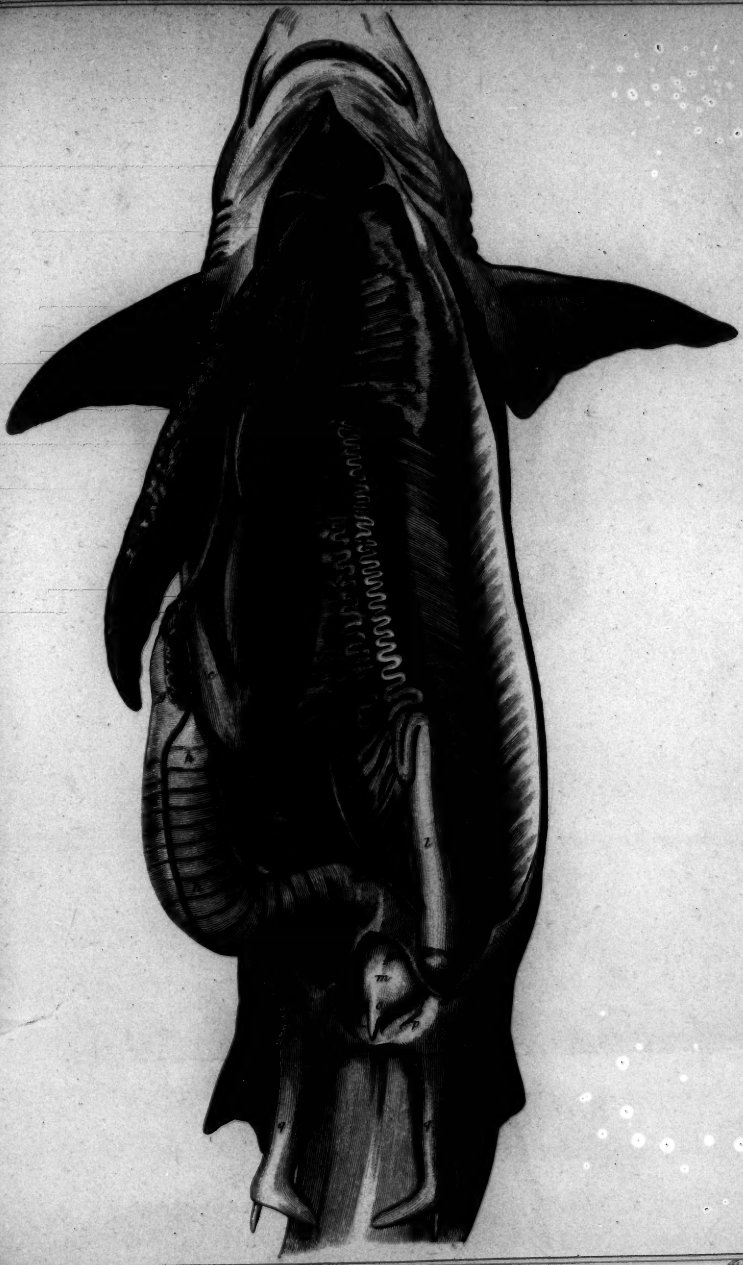
o The penis.

p p Two external openings leading into the cavity of the abdomen.

q q The two holders in their collapsed state.

PLATE X.

The holders in their extended state, as they appear when the male is clasping the female. In this state of the parts, the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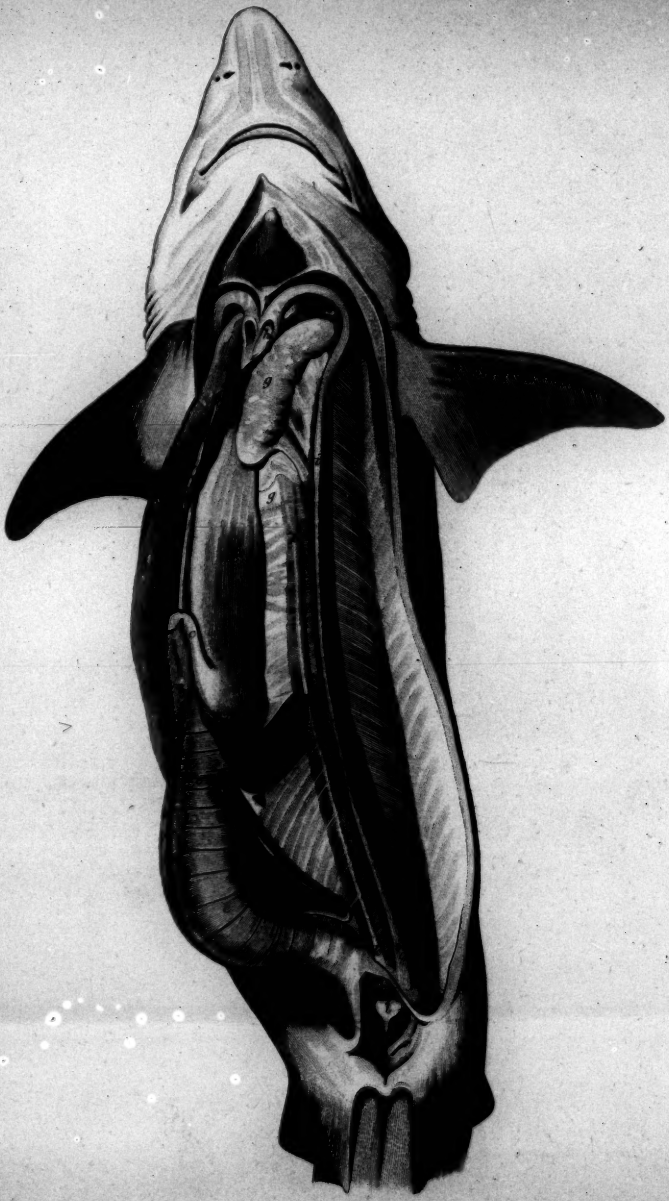

Fig. 1.

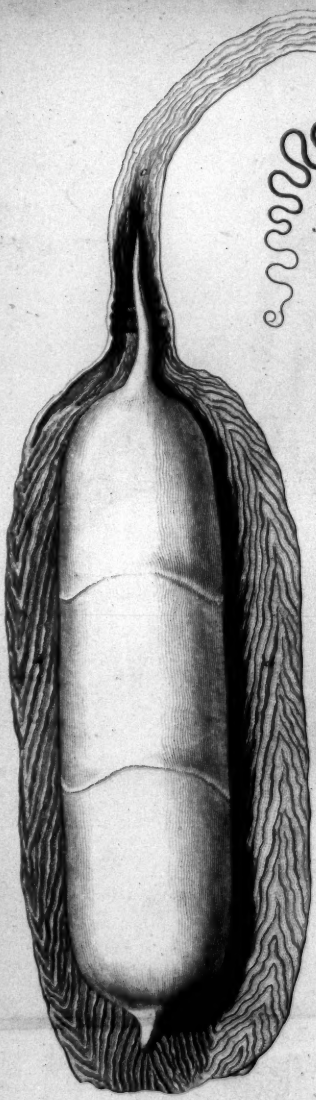

Fig. 3.

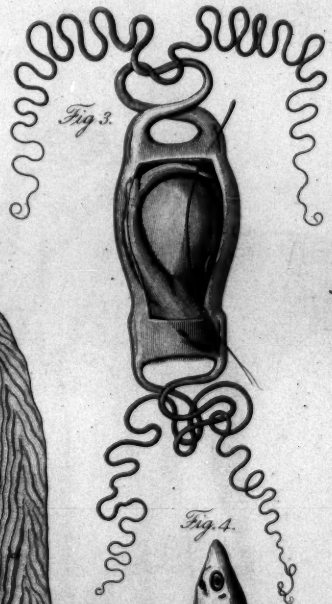

Fig. 2.

Fig. 4.

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penis is brought forward, and projects externally. On each side of it is seen the opening which leads into the cavity of the abdomen.

The parts are of the natural size.

Plate XI.

An internal view of the abdomen of the female squalus acanthias of Linnæus, to show the organs of generation in the maiden state.

a The heart.
b b The liver.
c c The stomach.
d The spleen.
e e The valvular intestine.
f The appendix.
g g The ovaria.
h h The openings of the oviducts, by which they receive the ova from the ovaria.
i i The course of the oviduct.
k k The oviduct on the opposite side.
l The clitoris.
m m The kidnies.

Plate XII.

Shows the oviduct of the squalus acanthias, with the ova in it, and the young fish after it has left the mother; also the ovum of the squalus canicula or oviparous dog-fish, and the young.

Fig. 1. The oviduct taken out of the body.

a The orifice which receives the ova, beyond which the oviduct is slit open.

b b The first portion of the oviduct.

c c The second portion.

d d The third portion, containing the ova inclosed in the capsule; they are three in number, beyond them the capsule comes to a point, and the oviducts are filled with a transparent jelly.

Fig. 8. The

Fig. 9. The [illegible] containing the young [illegible] it; and showing the slits by which the water [illegible] to the hemispheres of the young; the bag [illegible] comes through the other.

Fig. 10. The young [illegible]

PLATE XIII.

The [illegible] and lateral canals of the kanguroo, in the impregnated [illegible]

a a The [illegible]

b b The openings of the lateral canals into it.

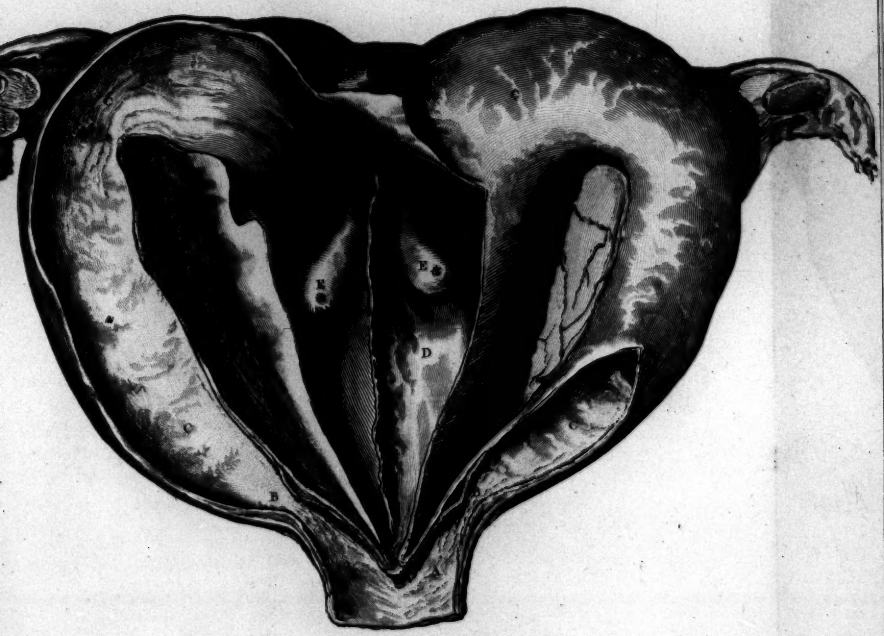
c c c c The two lateral canals, cut laid open.

d d The two sides of the uterus.

e e The openings of the fallopian tubes.

f The imperfect septum of the uterus.

g The ovarium slit open to shew the corpus luteum.



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XIII. *On Cystic Oxide, a new species of Urinary Calculus.* By William Hyde Wollaston, *M.D. Sec. R.S.*

Read July 5, 1810.

THE principal design of the present essay is to make known the existence, and to describe the leading properties of a new species of urinary calculus from the human bladder; but I shall at the same time take the opportunity of correcting an inaccuracy or two that I have observed in my former communication on this subject. (Phil. Trans. 1797.)

I, on that occasion, took notice of five kinds of urinary calculi,

1. The *lithic acid*, since called *uric acid*, originally analysed by SCHEELE.
2. The *oxalate of lime*, or *mulberry calculus*.
3. The phosphate of lime, or *bone-earth calculus*.
4. The ammoniacal phosphate of magnesia.
5. The *fusible calculus*, which consists of the two last species combined.

It is now about five years since I first met with another species, evidently differing from each of those before described. It was in the possession of Dr. REEVE of Norwich, who obligingly gave me a portion of it for the purpose of examining its chemical qualities. It had been taken from his brother when he was five years old, and at that time was covered with a coating of phosphate of lime very loose in its texture,

and consequently very soon separated.* This species is probably very rare; for, although I have omitted no opportunity of paying attention to any urinary concretions to which I could have access, I have, to this time, seen only one other specimen of the same substance. This last is in a collection of calculi belonging to GUY's Hospital, given by Mr. LUCAS, surgeon to that Institution, having been formed partly by his father, and partly by himself, in the course of their practise; and according to the present arrangement, (which, it is to be hoped, will not be altered) the calculus to which I allude may be found by reference to No. 46 of that collection. It was extracted by the usual operation, from a man 36 years of age, of whom no record is preserved, except that his name was WILLIAM SMALL. It weighed, when entire, 270 grains.

In appearance, these calculi resemble more nearly the triple phosphate of magnesia, than any other calculus; but they are more compact than that compound is usually found to be: not consisting of distinct laminæ, but appearing as one mass confusedly crystallized throughout its substance. Hence, instead of having the opacity and whiteness observable in fusible calculi, which consist of a number of small crystals cemented together, these calculi have a yellowish semi-transparency; and they have also a peculiar glistening lustre, like that of a body having a high refractive density.

When this substance is submitted to destructive distillation,

* I am informed, that another stone formed afterwards in the bladder of this boy, and that he died in consequence, without submitting to the operation a second time. The stone found in his bladder after death, consisted principally of uric acid, but was peculiar in one respect, as its center was hollow by the removal of some more soluble substance, of which the nucleus had consisted.

it yields fœtid carbonate of ammonia, partly fluid, and partly in a solid state, and a heavy fœtid oil, such as usually proceeds from animal substances; and there remains a black spongy coal, much smaller in proportion than is found after the distillation of uric calculi.

Under the blow-pipe it may be distinguished from uric acid by the smell, which at no period resembles that of prussic acid; but in addition to the usual smell of burned animal substances, there is a peculiar fœtor, of which I cannot give a correct idea, as I know no smell which it can be said to resemble.

This species of calculus is so readily acted upon by the generality of common chemical agents, that its character may perhaps be most distinctly marked, by an enumeration of those feeble powers that it can resist.

It is not dissolved (excepting in very small proportion) by water, by alcohol, by acetic acid, by tartaric acid, by citric acid, or by saturated carbonate of ammonia.

The solvents on the contrary, are far more numerous. It is dissolved, in considerable quantity, by muriatic acid, by nitric acid, by sulphuric acid, by phosphoric acid, and by oxalic acid.

It is also dissolved readily by pure alkaline menstrua: by potash, by soda, by ammonia, and by lime water. It is even dissolved by fully saturated carbonates of potash or of soda. Accordingly, these alkalies are not so convenient for the precipitation of this matter from acid solutions, as the carbonate of ammonia, which is not capable of redissolving the precipitate, though added in excess.

For a similar reason, the acids best suited for its precipita-

tion from alkaline solutions, are the acetic and citric acids. But the tartaric acid may occasion an appearance of precipitation, by forming a supertartrate with the alkali employed.

The combination of this substance with acids, may be made to crystallize without difficulty, and they form slender spicula radiating from a center, which readily dissolve again in water, unless they have been injured by being in any degree overheated.

The muriatic salt is decomposed by the heat of boiling water, on account of the volatility of the acid, and the rest are easily destroyed by a greater excess of heat.

The salt formed by combination with nitric acid, does not yield oxalic acid, and does not become red, as the uric acid does, when similarly treated; but it turns brown, becoming gradually darker, till it is ultimately black.

When the combinations with alkalies are evaporated, they leave small granular crystals; but as I was desirous of rendering my experiments as numerous as a limited quantity would permit, the portion which I could employ in any one experiment was too small for me to attempt to determine the form of such crystals.

When a hot solution in potash was neutralized by distilled vinegar, the precipitate did not immediately take place, but formed gradually during the cooling of the liquor in minute crystals, some at the surface of the fluid, and others attached to the sides of the vessel. The only definite form which I could observe, was that of flat hexagonal plates, but I could discern nothing which enabled me to judge of the primitive form of the crystal. On the surface of the calculus belonging to GUY'S Hospital, some minute crystals may be discerned, of

a different shape, being nearly cubic. And it is possible, that the hexagonal crystals may owe their figure to a small portion of alkali remaining in combination.

From the ready disposition of this substance to unite with both acids and alkalies, it would appear to be an oxide; and that it does, in fact, contain oxygen, is proved by the formation of carbonic acid in distillation. The quantity of oxygen present in the calculus is not, however, sufficient to give it acid properties, for it has no effect on paper coloured with litmus.

I am therefore inclined to consider it as an oxide; and since both the calculi that have yet been observed have been taken from the bladder, it may be convenient to give it the name of *cystic oxide*, which will serve to distinguish it from other calculi; and as this is unlike any other term at present employed in chemistry, it is to be hoped that it will not be thought to require any alteration.

Since the period of my first essay on gouty and urinary concretions, the general results contained in it have been confirmed by others, and I believe are incontrovertible. But I am under the necessity of acknowledging a mistake in the analysis of the mulberry calculus, though not of much importance. An acid is mentioned to have arisen by sublimation, and it was supposed to originate from a partial decomposition of the oxalic acid. But since pure oxalate of lime yields no such sublimate, it most probably arose from the mixture of a small quantity of uric acid in the calculus then under examination.

In the analysis of the triple phosphate of magnesia, there is another mistake of more consequence. In my selection

from numerous experiments for ascertaining the presence of phosphoric acid, I gave the preference to one in which nitrate of mercury was employed, on account of the facility of extracting the acid from the phosphate of mercury, by heat alone. But since the whole of the phosphoric acid is not precipitated by nitrate of mercury, sulphate of magnesia will not be formed on the addition of sulphuric acid, and the magnesia cannot be obtained separate by the same process.

It may have been in consequence of this over-sight, that a mistake on that subject has occurred in the succeeding volume of the Transactions.

A calculus is there described, which had been taken by Mr. THOMAS from the bladder of a dog, and a series of experiments are related, from which it was inferred to consist of super-phosphate of lime, and phosphate of ammonia. But from the appearance of this calculus (which was exhibited to the Society at the time when the paper was read) I was much inclined to think, that the nature of it was mistaken, and upon full consideration of the experiments, they did not appear to me conclusive.

I therefore obtained a portion of the calculus, and by the following process, the earth contained in it was proved to consist almost wholly of magnesia.

It was dissolved, with the exception of a very small residuum, by distilled vinegar.

The whole of the phosphoric acid was then precipitated by acetate of lead, added to excess.

The liquor was then poured off, and sulphuric acid was added, which precipitated the excess of lead, and at the same time formed sulphate of magnesia in solution.

By evaporation to dryness, the acetic acid was removed, and by subsequent increase of heat, the sulphate of ammonia, and excess of sulphuric acid were expelled.

The residuum being then dissolved in water, and the liquor suffered to crystallize by spontaneous evaporation, there remained a quantity of sulphate of magnesia, that weighed rather more than the quantity of calculus taken for the experiment.

It was evident, therefore, that in this instance, the calculus examined did not consist of super-phosphate of lime, and there is some reason to doubt, whether a compound, that is so very soluble in water, ever forms a part of urinary concretions.

Although the treatment of diseases is not in general, a fit subject to occupy the time of this Society, there is nevertheless, one suggestion, with respect to the prevention of calculous complaints, so nearly connected with my present subject, that I think it may deserve to be recorded.

Since the white matter contained in the urine of birds, which is voided along with their dung, has been remarked by M. VAUQUELIN, to consist principally of uric acid, I have paid some attention to the different proportion in which this matter is voided by different species of birds, to see how far it accorded with the different qualities of their food. And I found that in the dung of the goose, feeding wholly on grass, the proportion did not seem so much as $\frac{1}{200}$ of the whole dung. In that of a pheasant kept in a cage, and fed on barley alone, it was about $\frac{1}{14}$ part. In that of a hen, having the range of a garden and farm yard, and consequently procuring insects, and possibly other animal food, the proportion was manifestly much greater, and combined with lime. In the dung of a

hawk, fed upon flesh alone, the quantity of matter voided in a solid state bears but a small proportion to the residuum of uric acid, that is left by the urine when dry. And in the gannet feeding solely on fish, I have observed the evacuations in some instances to be mere urine, for it contained no solid matter, excepting the uric acid.

It seems consequently, deserving of inquiry, what changes might be produced in the urine of any one animal, by such alterations of diet, as its constitution would permit; for as far as any inference can be drawn from these varieties, which naturally occur, it would appear, that persons subject to calculi, consisting of uric acid, as well as gouty persons, in whom there is always a redundance of the same matter, have much reason to prefer vegetable diet, but that the preference usually given to fish above other kinds of animal food, is probably erroneous.

XIV. *Researches on the oxymuriatic Acid, its Nature and Combinations; and on the Elements of the muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution.** *By* H. Davy, *Esq. Sec. R.S. Prof. Chem. R. I. F.R.S.E.*

Read July 12, 1810.

THE illustrious discoverer of the oxymuriatic acid considered it as muriatic acid freed from hydrogene;† and the common muriatic acid as a compound of hydrogene and oxymuriatic acid; and on this theory he denominated oxymuriatic acid dephlogisticated muriatic acid.

M. BERTHOLLET,‡ a few years after the discovery of SCHEELE, made a number of important and curious experiments on this body; from which he concluded, that it was composed of muriatic acid gas and oxygene; and this idea for nearly twenty years has been almost universally adopted.

Dr. HENRY, in an elaborate series of experiments, made with the view of decomposing muriatic acid gas, ascertained that hydrogene was produced from it, by electricity; and he attributed the phænomenon to water contained in the gas.‖

In the Bakerian lecture for 1808, I have given an account of the action of potassium upon muriatic acid gas, by which more than one third of its volume of hydrogene is produced; and I have stated, that muriatic acid can in no instance be procured

* Communicated to the Royal Society at the request of the Managers of the Royal Institution.

† Mem. Acad. Stockholm for 1774, p. 94.

‡ Journal de Physique, 1785, p. 325.

‖ Phil. Trans. for 1800, p. 191.

from oxymuriatic acid, or from dry muriates, unless water or its elements be present.

In the second volume of the Memoires d'Arcueil, M. M. GAY LUSSAC and THENARD have detailed an extensive series of facts upon muriatic acid, and oxymuriatic acid. Some of their experiments are similar to those I have detailed in the paper just referred to; others are peculiarly their own, and of a very curious kind: their general conclusion is, that muriatic acid gas contains about one quarter of its weight of water; and that oxymuriatic acid is not decomposable by any substances but hydrogene, or such as can form triple combinations with it.

One of the most singular facts that I have observed on this subject, and which I have before referred to, is, that charcoal, even when ignited to whiteness in oxymuriatic or muriatic acid gases, by the VOLTAIC battery, effects no change in them; if it has been previously freed from hydrogene and moisture by intense ignition in vacuo.

This experiment, which I have several times repeated, led me to doubt of the existence of oxygene in that substance, which has been supposed to contain it above all others in a loose and active state; and to make a more rigorous investigation than had been hitherto attempted for its detection.

If oxymuriatic acid gas be introduced into a vessel exhausted of air, containing tin; and the tin be gently heated, and the gas in sufficient quantity, the tin and the gas disappear, and a limpid fluid, precisely the same as Libavius's liquor is formed;—it occured to me, that if this substance is a combination of muriatic acid and oxide of tin, oxide of tin ought to be separated from it by means of ammonia. I admitted ammoniacal gas over mercury to a small quantity of the liquor of Libavius;

it was absorbed with great heat, and no gas was generated; a solid result was obtained, which was of a dull white colour; some of it was heated, to ascertain if it contained oxide of tin; but the whole volatilized, producing dense pungent fumes.

Another experiment of the same kind, made with great care, and in which the ammonia was used in great excess, proved that the liquor of Libavius cannot be decompounded by ammonia; but that it forms a new combination with this substance.

I have described, on a former occasion, the nature of the operation of phosphorus on oxymuriatic acid, and I have stated that two compounds, one fluid, and the other solid, are formed in the process of combustion, of which the first, on the generally received theory of the nature of oxymuriatic acid, must be considered as a compound of muriatic acid and phosphorous acid, and the other of muriatic acid and phosphoric acid. It occured to me, that if the acids of phosphorus really existed in these combinations, it would not be difficult to obtain them, and thus to gain proofs of the existence of oxygene in oxymuriatic acid.

I made a considerable quantity of the solid compound of oxymuriatic acid and phosphorus by combustion, and saturated it with ammonia, by heating it in a proper receiver filled with ammoniacal gas, on which it acted with great energy, producing much heat; and they formed a white opaque powder. Supposing that this substance was composed of the dry muriates and phosphates of ammonia; as muriate of ammonia is very volatile, and as ammonia is driven off from phosphoric acid, by a heat below redness, I conceived that by igniting the product obtained, I should procure phosphoric acid; I therefore introduced some of the powder into a tube of green glass, and

heated it to redness, out of the contact of air by a spirit lamp; but found, to my great surprise, that it was not at all volatile nor decomposable at this degree of heat, and that it gave off no gaseous matter.

The circumstance that a substance composed principally of oxymuriatic acid, and ammonia, should resist decomposition or change at so high a temperature, induced me to pay particular attention to the properties of this new body.

It had no taste nor smell; it did not seem to be soluble, nor did it undergo any perceptible change when digested in boiling water: it did not appear to be acted upon by sulphuric, muriatic, or nitric acids, nor by a strong lixivium of potash. The only processes by which it seemed susceptible of decomposition were by combustion, or the action of ignited hydrat of potash. When brought into the flame of a spirit lamp and made red-hot, it gave feeble indications of inflammation, and tinged the flame of a yellow colour, and left a fixed acid, having the properties of phosphoric acid. When acted on by red-hot hydrat of potash, it emitted a smell of ammonia, burnt where it was in contact with air, and appeared to dissolve in the alkali. The potash which had been so acted upon gave muriatic acid, by the addition of sulphuric acid.

I heated some of the powder to whiteness, in a tube of platina; but it did not appear to alter; and after ignition gave ammonia by the action of fused hydrat of potash.

I caused ammonia, made as dry as possible, to act on the phosphuretted liquor of M. M. GAY LUSSAC and THENARD; and on the sulphuretted muriatic liquor of Dr. THOMSON; but no decomposition took place; nor was any muriate of ammonia formed when proper precautions were taken to exclude mois-

ture. The results were new combinations; that from the phosphoretted liquor, was a white solid, from which a part of the phosphorus was separated by heat; but which seemed no further decomposable, even by ignition. That from the sulphuretted liquor was likewise solid, and had various shades of colour, from a bright purple to a golden yellow, according as it was more or less saturated with ammonia; but as these compounds did not present the same uniform and interesting properties, as that from the phosphoric sublimate, I did not examine them minutely: I contented myself by ascertaining that no substance known to contain oxygene could be procured from oxymuriatic acid, in this mode of operation.

It has been said, and taken for granted by many chemists, that when oxymuriatic acid and ammonia act upon each other, water is formed; I have several times made the experiment, and I am convinced that this is not the case. When about 15 or 16 parts of oxymuriatic acid gas are mixed with from 40 to 45 parts of ammoniacal gas, there is a condensation of nearly the whole of the acid and alkaline gasses, and from 5 to 6 parts of nitrogene are produced; and the result is dry muriate of ammonia.

Mr. Cruikshank has shown that oxymuriatic acid and hydrogene, when mixed in proportions nearly equal, produce a matter almost entirely condensible by water; and M. M. Gay Lussac and Thenard, have stated that this matter is common muriatic acid gas, and that no water is deposited in the operation. I have made a number of experiments on the action of oxymuriatic acid gas and hydrogene. When these bodies were mixed in equal volumes over water, and introduced into an exhausted vessel and fired by the electric spark, there was always a deposition of a slight vapour, and a conden-

sation of from $\frac{1}{15}$ to $\frac{1}{20}$ of the volume; but the gas remaining was muriatic acid gas. I have attempted to make the experiment in a manner still more refined, by drying the oxymuriatic acid and the hydrogene by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures; but I have never been able to avoid a slight condensation; though in proportion as the gasses were free from oxygene or water, this condensation diminished.

I mixed together sulphuretted hydrogene in a high degree of purity and oxymuriatic acid gas both dried, in equal volumes: in this instance the condensation was not $\frac{1}{40}$; sulphur, which seemed to contain a little oxymuriatic acid, was formed on the sides of the vessel; no vapour was deposited; and the residual gas contained about $\frac{19}{20}$ of muriatic acid gas, and the remainder was inflammable.

M. M. GAY LUSSAC and THENARD have proved by a copious collection of instances, that in the usual cases where oxygene is procured from oxymuriatic acid, water is always present, and muriatic acid gas is formed; now, as it is shewn that oxymuriatic acid gas is converted into muriatic acid gas, by combining with hydrogene, it is scarcely possible to avoid the conclusion, that the oxygene is derived from the decomposition of water, and, consequently, that the idea of the existence of water in muriatic acid gas, is hypothetical, depending upon an assumption which has not yet been proved—the existence of oxygene in oxymuriatic acid gas.

M. M. GAY LUSSAC and THENARD indeed have stated an experiment, which they consider as proving that muriatic acid gas contains one quarter of its weight of combined water. They passed this gas over litharge, and obtained so much water; but it is obvious that in this case they formed the same

compound as that produced by the action of oxymuriatic acid on lead; and in this process the muriatic acid must loose its hydrogene, and the lead its oxygene; which of course would form water; these able chemists, indeed, from the conclusion of their memoir, seem aware, that such an explanation may be given, for they say that the oxymuriatic acid *may be* considered as a simple body.

I have repeated those experiments which led me first to suspect the existence of combined water in muriatic acid, with considerable care; I find that, when mercury is made to act upon 1 in volume of muriatic acid gas, by VOLTAIC electricity, all the acid disappears, calomel is formed, and about .5 of hydrogene evolved.

With potassium, in experiments made over very dry mercury, the quantity of hydrogene is always from 9 to 11, the volume of the muriatic acid gas used being 20.

And in some experiments made very carefully by my brother Mr. JOHN DAVY, on the decomposition of muriatic acid gas, by heated tin and zinc, hydrogene equal to about half its volume was disengaged, and metallic muriates, the same as those produced by the combustion of tin and zinc in oxymuriatic gas, resulted.

It is evident from this series of observations, that SCHEELE's view, (though obscured by terms derived from a vague and unfounded general theory,) of the nature of the oxymuriatic and muriatic acids, may be considered as an expression of facts; whilst the view adopted by the French school of chemistry, and which, till it is minutely examined, appears so beautiful and satisfactory, rests in the present state of our knowledge, upon hypothetical grounds.

When oxymuriatic acid is acted upon by nearly an equal volume of hydrogene, a combination takes place between them, and muriatic acid gas results. When muriatic acid gas is acted on by mercury, or any other metal, the oxymuriatic acid is attracted from the hydrogene, by the stronger affinity of the metal; and an oxymuriate, exactly similar to that formed by combustion, is produced.

The action of water upon those compounds, which have been usually considered as muriates, or as dry muriates, but which are properly combinations of oxymuriatic acid with inflammable bases, may be easily explained, according to these views of the subject. When water is added in certain quantities to Libavius's liquor, a solid crystallized mass is obtained, from which oxide of tin and muriate of ammonia can be procured by ammonia. In this case, oxygene may be conceived to be supplied to the tin, and hydrogene to the oxymuriatic acid.

The compound formed by burning phosphorus in oxymuriatic acid, is in a similar relation to water: if that substance be added to it, it is resolved into two powerful acids; oxygene, it may be supposed, is furnished to the phosphorus to form phosphoric acid, hydrogene to the oxymuriatic acid to form common muriatic acid gas.

None of the combinations of the oxymuriatic acid with inflammable bodies, can be decomposed by dry acids; and this seems to be the test which distinguishes the oxymuriatic combinations from the muriates, though they have hitherto been confounded together. Muriate of potash for instance, if M. BERTHOLLET's estimation of its composition, approaches towards accuracy, when ignited, is a compound of oxymu-

riatic acid with potassium; muriate of ammonia, is a compound of muriatic acid gas and ammonia; and when acted on by potassium, it is decompounded; the oxymuriatic acid may be conceived to combine with the potassium to form muriate of potash, and the ammonia and hydrogene are set free.

The vivid combustion of bodies in oxymuriatic acid gas, at first view, appears a reason why oxygene should be admitted in it; but heat and light are merely results of the intense agency of combination. Sulphur and metals, alkaline earths and acids become ignited during their mutual agency; and such an effect might be expected in an operation so rapid, as that of oxymuriatic acid upon metals and inflammable bodies.

It may be said, that a strong argument in favour of the hypothesis, that oxymuriatic acid consists of an acid basis united to oxygene, exists in the general analogy of the compounds of oxymuriatic acid and metals, to the common neutral salts; but this analogy when strictly investigated, will be found to be very indistinct, and even allowing it, it may be applied with as much force to support an opposite doctrine, namely, that the neutral salts are compounds of bases with water; and the metals of bases with hydrogene; and that in the case of the action of oxymuriatic acid and metals, the metal furnishes hydrogene to form muriatic acid, and a basis to produce the neutral combination.

That the quantity of hydrogene evolved during the decomposition of muriatic acid gas by metals, is the same that would be produced during the decomposition of water by the same bodies, appears, at first view, an evidence in favour of the existence of water in muriatic acid gas; but as there is only one known combination of hydrogene with oxymuriatic acid,

one quantity must always be separated. Hydrogene is disengaged from its oxymuriatic combination, by a metal, in the same manner as one metal is disengaged by another, from similar combinations; and of all inflammable bodies that form compounds of this kind, except perhaps phosphorus and sulphur, hydrogene is that which seems to adhere to oxymuriatic acid with the least force.

I have caused strong explosions from an electrical jar, to pass through oxymuriatic gas, by means of points of platina, for several hours in succession; but it seemed not to undergo the slightest change.

I electrized the oxymuriates of phosphorus and sulphur for some hours, by the power of the VOLTAIC apparatus of 1000 double plates; no gas separated, but a minute quantity of hydrogene, which I am inclined to attribute to the presence of moisture in the apparatus employed; for I once obtained hydrogene from Libavius's liquor by a similar operation; but I have ascertained, that this was owing to the decomposition of water, adhering to the mercury; and in some late experiments made with 2000 double plates, in which the discharge was from platina wires, and in which the mercury used for confining the liquor was carefully boiled, there was no production of any permanent elastic matter.

As there are no experimental evidences of the existence of oxygene in oxymuriatic acid gas, a natural question arises, concerning the nature of these compounds, in which the muriatic acid has been supposed to exist, combined with much more oxygene than oxymuriatic acid, in the state in which it has been named by Mr. CHENEVIX, hyperoxygenized muriatic acid.

Can the oxymuriatic *acid* combine either with oxygene or hydrogene, and form with each of them an acid compound; of which that with hydrogene has the strongest, and that with oxygene the weakest affinity for bases? for the able chemist to whom I have just referred, conceives that hyperoxymuriates are decomposed by muriatic acid. Or, is hyperoxymuriatic acid the basis of all this class of bodies, the most simple form of this species of matter?

The phænomena of the composition and decomposition of the hyperoxymuriates, may be explained on either of these suppositions; but they are mere suppositions unsupported by experiment.

I have endeavoured to obtain the neutralizing acid, which has been imagined to be hyperoxygenised, from hyperoxymuriate of potash, by various modes, but uniformly without success. By distilling the salt with dry boracic acid, though a little oxymuriatic acid is generated, yet oxygene is the chief gaseous product, and a muriate of potash not decomposable is produced.

The distillation of the orange coloured fluid, produced by dissolving hyperoxymuriate of potash in sulphuric acid, affords only oxygene in great excess, and oxymuriatic acid.

When solutions of muriates, or muriatic acid are electrized in the VOLTAIC circuit, oxymuriatic acid is evolved at the positive surface, and hydrogene at the negative surface. When a solution of oxymuriatic acid in water is electrized, oxymuriatic acid and oxygene appear* at the positive surface,

* The quantity of oxymuriatic acid in the aqueous solution, is so small, that the principal products must be referred to the decomposition of water. This happens in other instances; the water only is decomposed in dilute solutions of nitric and sulphuric acids.

and hydrogene at the negative surface, facts which are certainly unfavourable to the idea of the existence of hyperoxygenised muriatic acid, whether it be imagined a compound of oxymuriatic acid with oxygene, or the basis of oxymuriatic acid.

If the facts respecting the hyperoxymuriate of potash, indeed, be closely reasoned upon, it must be regarded as nothing more than as a triple compound of oxymuriatic acid, potassium, and oxygene. We have no right to assume the existence of any peculiar acid in it, or of a considerable portion of combined water; and it is perhaps more conformable to the analogy of chemistry, to suppose the large quantity of oxygene combined with the potassium, which we know has an intense affinity for oxygene, and which from some experiments, I am inclined to believe, is capable of combining directly with more oxygene than exists in potash, than with the oxymuriatic acid, which, as far as is known, has no affinity for that substance.

It is generally supposed that a mixture of oxymuriatic acid and hyperoxymuriatic acid is disengaged when hyperoxymuriate of potash is decomposed by common muriatic acid;* but I am satisfied from several trials, that the gas procured in this way, when not mixed with oxygene, unites to the same quantity of hydrogene,† as common oxymuriatic acid gas from manganese; and I find, by a careful examination, that the gas disengaged during the solution of platina, in a mixture

* If hyperoxymuriate of potash be decomposed by nitric or sulphuric acid, it affords oxymuriatic acid and oxygene. If it be acted upon by muriatic acid, it affords a large quantity of oxymuriatic acid gas only. In this last case, the phænomenon seems merely to depend upon the decomposition of the muriatic acid gas, by the oxygene, loosely combined in the salt.

† This likewise appears from Mr. CRUICKSHANK'S experiments. See Nicholson's Journal, Vol. V. 4to. p. 206.

of nitric and muriatic acids, which has been regarded as hyper-oxymuriatic acid, but which I stated some years ago to possess the properties of oxymuriatic acid gas,* is actually that body, owing its peculiar colour to a small quantity of nitromuriatic vapour suspended in it, and from which it is easily freed by washing.

Few substances, perhaps, have less claim to be considered as acid, than oxymuriatic acid. As yet we have no right to say that it has been decompounded; and as its tendency of combination is with pure inflammable matters, it may possibly belong to the same class of bodies as oxygene.

May it not in fact be a *peculiar* acidifying and dissolving principle, forming compounds with combustible bodies, analogous to acids containing oxygene, or oxides, in their properties and powers of combination; but differing from them, in being for the most part, decomposable by water? On this idea muriatic acid may be considered as having hydrogene for its basis, and oxymuriatic acid for its acidifying principle. And the phosphoric sublimate as having phosphorus for its basis, and oxymuriatic acid for its acidifying matter. And Libavius's liquor, and the compounds of arsenic with oxymu-

* The platina, I find by several experiments, made with great care, has no share in producing the evolution of this gas. It is formed during the production of aqua regia. The hydrogene of the muriatic acid attracts oxygene from the nitric acid. Oxymuriatic acid gas is set free, and nitrous gas remains in the solution, and gives it a deep red colour. *Nitrous* acid and muriatic acid produce no oxymuriatic acid gas. Platina, during its solution in perfectly formed aqua regia, gives only nitrous gas and nitrous vapour; and I find, that rather more oxymuriatic acid gas is produced, by heating together equal quantities of nitric acid of 1.45, and muriatic acid of 1.18, when they are not in contact with platina, than when exposed to that metal. The oxymuriatic acid gas, produced from muriatic acid by nitric acid, I find combines with about an equal volume of hydrogene by detonation.

riatic acid, may be regarded as analogous bodies. The combinations of oxymuriatic acid with lead, silver, mercury, potassium, and sodium, in this view would be considered as a class of bodies related more to oxides than acids, in their powers of attraction.

It is needless to take up the time of this learned Society by dwelling upon the imperfection of the modern nomenclature of these substances. It is in many cases connected with false ideas of their nature and composition, and in a more advanced state of the enquiry, it will be necessary for the progress of science, that it should undergo material alterations.

It is extremely probable that there are many combinations of the oxymuriatic acid with inflammable bodies which have not been yet investigated. With phosphorus it seems capable of combining in at least three proportions; the phosphuretted muriatic acid of GAY LUSSAC and THENARD is the compound containing the maximum of phosphorus. The chrystalline phosphoric sublimate, and the liquor formed by the combustion of phosphorus in oxymuriatic acid gas, disengage no phosphorus by the action of water; the sublimate, as I have already mentioned, affords phosphoric and muriatic acid; and the liquid, I believe only phosphorous acid and muriatic acid.

The sublimate from the boracic basis gives, I believe, only boracic and muriatic acid, and may be regarded as boracium acidified by oxymuriatic acid.

It is evident, that whenever an oxymuriatic combination is decomposed by water, the oxide or acid or alkali or oxidated body formed must be in the same proportion as the muriatic acid gas, as the oxygene and hydrogene must bear the same relation to each other; and experiments upon these com-

pounds will probably afford simple modes of ascertaining the proportions of the elements, in the different oxides, acids, and alkaline earths.

If, according to the ingenious idea of Mr. DALTON, hydrogene be considered as 1 in weight, in the proportion it exists in water, then oxygene will be nearly 7.5; and assuming that potash is composed of 1 proportion of oxygene, and 1 of potassium, then potash will be 48, and potassium * about 40.5; and from an experiment which I have detailed in the last Bakerian lecture, on the combustion of potassium in muriatic acid gas, oxymuriatic acid will be represented by 32.9, and muriatic acid gas, of course, by 33.9; and this estimation agrees with the specific gravity of oxymuriatic acid gas, and muriatic acid gas. From my experiments, 100 cubical inches of oxymuriatic acid gas weigh, the reductions being made for the mean temperature and pressure, 74.5 grains; whereas by estimation they should weigh 74.6. Muriatic acid gas I find weighs, under like circumstances, in the quantity of 100 cubic inches, 39 grains; by estimation it should weigh 38.4 grains.

It is easy from these data, knowing the composition of any dry muriate, to ascertain the quantity of oxide or of acid it would furnish by the action of water, and consequently the quantity of oxygene with which the inflammable matter will combine.†

* Supposing potash to contain nearly 15.6 per cent. of oxygene.

† I have stated in the last Bakerian lecture, that during the decomposition of the amalgam from ammonia, 1 in volume of hydrogene to 2 of ammonia is evolved: it is remarkable, that whatever theory of the nature of this extraordinary compound be adopted, there will be a happy coincidence as to definite proportions. If it be supposed that the hydrogene arises from the decomposition of water; then the oxygene that

In considering the dry muriates, as compounds of oxymuriatic acid and inflammable bodies; the argument that I have used in the last Bakerian lecture, to shew that potassium does not form hydrate of potash by combustion, is considerably strengthened; for from the quantity of oxymuriatic acid the metal requires to produce a muriate, it seems to be shewn that it is the simplest known form of the alkaline matter. This I think approaches to an experimentum crucis. Potash made by alcohol, and that has been heated to redness, appears to be an hydrat of potash, whilst the potash formed by the combustion of potassium must be considered as a pure metallic oxide, which requires about 19 per cent. of water to convert into a hydrat.

Amongst all the known combustible bodies, charcoal is the only one which does not combine directly with oxymuriatic acid gas; and yet there is reason for believing that this com-

must be assumed to exist in ammonia, will be exactly sufficient to neutralize the hydrogene, in an equal volume of muriatic acid; or if it be said that ammonium is a compound of 2 of ammonia and 1 of hydrogene in volume, then equal volumes of muriatic acid gas and ammonia will produce the same compound as oxymuriatic acid and ammonium, supposing they could be immediately combined. I once thought that the phænomena of metallization might be explained according to a modified phlogistic theory, by supposing three different classes of metallic bodies: First, The metal of ammonia, in which hydrogene was so loosely combined as to be separable with great ease, and which, in consequence of the small affinity of the basis for water, it had little tendency to combine with oxygene. The second, the metals of the alkalies and alkaline earths, in which the hydrogene was more firmly combined, but in combustion, forming water capable of being separated from the basis. And, thirdly, the metals of the earths and common metals, in which the hydrogene was more intimately combined; producing by union with oxygene, water not separable by any new attractions. The phænomena of the action of potassium and sodium upon muriatic acid, referred to in the text, seem however to overturn these speculations so far as they concern the metals from the fixed alkalies.

bination may be formed by the intermedium of hydrogene. I am inclined to consider the oily substance produced by the action of oxymuriatic acid gas, and olefiant gas, as a ternary compound of these bodies; for they combine nearly in equal volumes; and I find that, by the action of potassium upon the oil so produced, muriate of potash is formed, and gaseous matter, which I have not yet been able to collect in sufficient quantity to decide upon its nature, is formed. Artificial camphor, and muriatic ether, as is probable from the ingenious experiments of M. GEHLEN and M. THENARD, must be combinations of a similar kind, one probably with more hydrogene, and the other with more carbon.

One of the greatest problems in œconomical chemistry, is the decomposition of the muriates of soda and potash. The solution of this problem will, perhaps, be facilitated by these new views. The affinity of potassium and sodium for oxymuriatic acid, is very strong; but so likewise is their attraction for oxygene, and the affinity of their oxides for water. The affinities of oxymuriatic acid gas for hydrogene, and of muriatic acid gas for water, are likewise of a powerful kind. Water, therefore, should be present in all cases, when it is intended to attempt to produce alkali. It is not difficult after these views to explain the decomposition of common salt, by aluminous or silicious substances, which, as it has been long known, act only when they contain water. In these cases the sodium may be conceived to combine with the oxygene of the water and with the earth, to form a vitreous compound; and the oxymuriatic acid to unite with the hydrogene of the water, forming muriatic acid gas.

It is also easy, according to these new ideas, to explain

the decomposition of salt by moistened litharge, the theory of which has so much perplexed the most acute chemists. It may be conceived to be an instance of compound affinity: the oxymuriatic acid is attracted by the lead, and the sodium combines with the oxygene of the litharge and with water to form hydrat of soda, which gradually attracts carbonic acid from the air.

As iron has a strong affinity for oxymuriatic acid, I attempted, to procure soda by passing steam over a mixture of iron filings, and muriate of soda intensely heated: and in this way, I succeeded in decomposing some of the salt: hydrogene came over; a little hydrate of soda was formed; and muriate of iron was produced.

It does not seem improbable, supposing the views that have been developed accurate, that by complex affinities, even potassium and sodium in their metallic form, may be procured from their oxymuriatic combinations: for this purpose the oxymuriatic acid should be attracted by one substance, and the alkaline metals by another; and such bodies should be selected for the experiment, as would produce compounds differing considerably in degree of volatility.

I cannot conclude the subject of the application of these doctrines, without asking permission to direct the attention of the Society, to some of the theoretical relations of the facts noticed in the preceding pages.

That a body principally composed of oxymuriatic acid and ammonia, two substances which have been generally conceived incapable of existing together, should be so difficult of decomposition, as to be scarcely affected by any of the agents of chemistry, is a phænomenon of a perfectly new kind.

Three bodies, two of which are permanent gases, and the other of which is considerably volatile, form in this instance, a substance neither fusible nor volatile, at a white heat. It could not have been expected that ammonia would remain fixed at such a temperature; but that it should remain fixed in combination with oxymuriatic acid, would have appeared incredible, according to all the existing analogies of chemistry. The experiments on which these conclusions are founded, are, however, uniform in their results: and it is easy to repeat them. They seem to shew, that the common chemical proposition, that complexity of composition is uniformly connected with facility of decomposition, is not well founded. The compound of oxymuriatic acid, phosphorus, and ammonia, resembles an oxide, such as silex, or that of columbium in its general chemical characters, and is as refractory when treated by common re-agents; and except by the effects of combustion, or the agency of fused potash, its nature could not be detected by any of the usual methods of analysis. Is it not likely, reasoning from these circumstances, that many of the substances, now supposed to be elementary, may be reduced into simpler forms of matter? And that an intense attraction, and an equilibrium of attraction, may give to a compound, containing several constituents, that refractory character, which is generally attributed to unity of constitution, or to the homogeneous nature of its parts?

Besides the compound of the phosphoric sublimate and ammonia, and the other analogous compounds which have been referred to, it is probable that other compounds of like nature may be formed of the oxides, alkalies, and earths, with the oxymuriatic combinations, or of the oxymuriatic

compounds with each other; and should this be the case, the more refined analogies of chemical philosophy will be extended by these new, and as it would seem at first view, contradictory facts. For if, as I have said, oxymuriatic acid gas be referred to the same class of bodies as oxygene gas, then, as oxygene is not an acid, but forms acids by combining with certain inflammable bodies, so oxymuriatic acid, by uniting to similar substances, may be conceived to form either acids, which is the case when it combines with hydrogene, or compounds like acids or oxides, capable of forming neutral combinations, as in the instances of the oxymuriates of phosphorus and tin.

Like oxygene, oxymuriatic acid is attracted by the positive surface in VOLTAIC combinations; and on the hypothesis of the connection of chemical attraction with electrical powers, all its energies of combination correspond with those of a body supposed to be negative in a high degree.

And in most of its compounds, except those containing the alkaline metals, which may be conceived in the highest degree positive, and the metals with which it forms insoluble compounds, it seems still to retain its negative character.

I shall occupy the time of the Society for a few minutes longer only, for the purpose of detailing a few observations connected with the Bakerian lectures, delivered in the two last years; particularly those parts of them relating to sulphur and phosphorus, which new and more minute enquiries have enabled me to correct or extend.

I have already mentioned that there are considerable differences in the results of experiments, made on the action of potassium, on sulphur and phosphorus, and their combinations

with hydrogene, according to different circumstances of the process. I shall now refer to such of these circumstances as I have been able fully to investigate.

The able researches of Dr. THOMSON have shewn that sulphur, in its usual state, contains small quantities of acid matter; and though, in my first experiments, I conceived that by employing chrystalized native sulphur, which had been recently sublimed in nitrogene, I should avoid the presence of any foreign matter, yet I am inclined to believe that this is not the case; for by subliming some similar sulphur in nitrogene, I find that litmus paper placed in the upper part of the retort is slightly reddened.

When potassium is made to unite with sulphur, if the retort employed is not lined with sulphur, some of the potassium is destroyed by acting upon the glass; and when large quantities of sulphur are used, it is very difficult to decompose the whole of the sulphuret of potassium by an acid: sulphuretted hydrogene likewise is soluble in muriatic acid; and this circumstance led me to under-rate the quantity of sulphuretted hydrogene given off in experiments of this kind.*

In acting upon sulphuretted hydrogene by potassium in my early experiments, I used large quantities of the gas and of the metal; and in these cases I have reason to believe that the violence of the combustion, occasioned the decomposition

* This circumstance has been pointed out by M. M. GAY LUSSAC and THENARD; in a paper printed in the Journal de Physique for December, in which these gentlemen endeavour to shew that, whether potassium has been acted upon by large or small quantities of sulphur, and under all circumstances, it evolves a quantity of gas exactly equal to that which it produces by the action of water. I have been able to gain no results so precise on this subject. I have in another place (the same journal in which their memoir has appeared) offered some observations on their enquiries.

of a considerable quantity of the gas; and, in consequence, led me to form erroneous conclusions concerning the nature of this curious operation.

In all late experiments in which sulphur, or sulphuretted hydrogene were concerned, I have used muriatic acid saturated with sulphuretted hydrogene over mercury. I have employed sulphur distilled from iron pyrites in vacuo, which did not in the slightest degree affect litmus paper, and I have combined it with potassium in retorts of green-glass, or plate-glass lined with sulphur and filled with very pure nitrogene or hydrogene. In making potassium act upon sulphuretted hydrogene, I have employed the gas only in the quantities of from 1 to 3 cubical inches, and have made the combination in narrow curved tubes of green glass over dry mercury. With all these precautions, and after having made a great number of experiments, I am not able to gain perfectly uniform results. Yet there is a sufficient correspondence between them to enable me to form conclusions, which I may venture to say cannot be far from the truth.

When 1 grain of potassium, which would give by the action of water about 1 cubical inch and $\frac{1}{10}$ of hydrogene is made to act upon about half a grain of sulphur, some sulphur sublimes during the combination, which always takes place with heat and light, and from $\frac{1}{14}$ to $\frac{1}{10}$ of a cubical inch of sulphuretted hydrogene is evolved. The compound acted on by muriatic acid, saturated with sulphuretted hydrogene, affords from $\frac{9}{10}$ to $\frac{11}{10}$ of a cubical inch of pure sulphuretted hydrogene.

When more sulphur is used so as to be from twice to ten times the weight of the potassium, the quantity of sulphu-

retted hydrogene evolved by the action of the acid, is from $\frac{7}{10}$ to $\frac{9}{10}$; but if heat be applied to the combination, so as to drive off the superfluous sulphur, the quantity of gas collected is very little inferior to that produced from the combination in which a small proportion of sulphur is used; and I am inclined to believe, from the phænomena presented in a great number of experiments, that sulphur and potassium, when heated together under common circumstances, combine only in one proportion, in which the metal is to the sulphur nearly as 3 to 1 in weight; and in which the quantities are such that the compound burns into neutral sulphate of potash.

When a grain of potassium is made to act upon about 1.1 cubical inches of sulphuretted hydrogene, all the hydrogene is set free, and a sulphuret of potassium containing one fourth of sulphur is formed, exactly the same as that produced by the immediate combination of sulphur and the metal.

When sulphuretted hydrogene is employed in larger quantities, there is an absorption of this gas, and a volume is taken up about equal to the quantity of hydrogene disengaged, and a compound of sulphuretted hydrogene and sulphuret of potash is formed, which gives sulphuretted hydrogene by the action of an acid, nearly double in quantity to that given by the sulphuret of potassium.

From a number of experiments I am inclined to believe that potassium and phosphorus, in whatever quantities they are heated together, combine only in one proportion, a grain of potassium requiring about $\frac{1}{8}$ of a grain of phosphorus to form a phosphuret; which when acted upon by muriatic acid, produces from $\frac{8}{10}$ to $\frac{10}{10}$ of a cubical inch of phosphuretted hydrogene.

Half a grain of potassium decomposes nearly three cubical inches of phosphuretted hydrogene, and sets free rather more than four cubical inches of hydrogene; and the phosphuret formed seems to be of the same kind as that produced by direct combination of the metal with phosphorus.

If, according to Mr. DALTON's ideas of proportion, the quantity in which sulphur enters into its combinations were to be deduced from its union with potassium, in which it seems to form about $\frac{1}{4}$ the weight of the compound, the number representing it would be 13.5. I have lately weighed sulphuretted hydrogene, and sulphureous acid gas, with great care: the specific gravity of the first at mean temperature and pressure, from my experiments, is 10645, which differs very little from the estimation of Mr. KIRWAN: that of sulphureous acid gas I find is 20967. Sulphuretted hydrogene, as I have shown, contains an equal volume of hydrogene; and on this datum the number representing sulphur is 13.4. I have never been able to burn sulphur in oxygene without forming sulphuric acid in small quantities; but in several experiments I have obtained from 92 to 98 parts of sulphureous acid from 100 of oxygene in volume; from which I am inclined to believe, that sulphureous acid consists of sulphur dissolved in an equal volume of oxygene; which would give the number as 13.7* nearly, considering the acid gas as containing 1 pro-

* The estimation from the composition of sulphuretted hydrogene, must be considered as most accurate, and that from the formation of the sulphuret of potassium as least accurate: for it was only by combining sulphur and potassium in small proportions, and ascertaining in what cases uncombined sulphur could be distilled from the compound, that I gained my conclusions concerning the composition of the sulphuret of potassium.

In the last Bakerian lecture, I have estimated the specific gravity of sulphuretted

portion of sulphur, and 2 of oxygene; and these estimations do not differ from each other materially.

I have made several experiments on the combustion of phosphorus in oxygene gas. From the most accurate, I am inclined to conclude that 25 of phosphorus absorb in combustion about 34 of oxygene in weight; and considering phosphoric acid as composed of 3 proportions of oxygene and 1 of phosphorus, the number representing phosphorus will be about 16.5, which is not very remote from the number that may be deduced from the composition of phosphoret of potassium.

The numbers which represent the proportions in which sulphur and phosphorus unite with other bodies, are such, as do not exclude the existence of combined portions of oxygene and hydrogene in their constitution; but it may be questioned, whether the opinion which I formed, that the inflammable gas disengaged from them by electricity, is necessary to the peculiar form in which these bodies exist, is not erroneous. Phosphorus, as I have stated in the last Bakerian lecture, is capable of forming a solid hydruret: and a part of the sulphur distilled from iron pyrites, is usually of a soft consistence, and emits the smell of sulphuretted hydrogene, and probably contains that body. It is not unlikely, that in all cases, phosphorus and sulphur contain small quantities of the hydrurets of phosphorus and sulphur; and the production of a minute portion of sulphuric acid in the slow combustion of sulphur, is pro-

hydrogene at 35 grains, the 100 cubical inches, which was not far from the mean, between the estimations of Mr. KIRWAN and Mr. THENARD. According to this last experiment, sulphuretted hydrogene is composed of 1 proportion of hydrogene, represented by 1, and 1 of sulphur represented by 13.4

bably connected with the production of water. Though the pure oxides of sulphur and phosphorus have never been obtained, yet from the doctrine of definite proportions, these bodies ought, under certain circumstances, to be formed. And I am inclined to believe, that they sometimes exist in minute quantities, in common phosphorus and sulphur, and with hydrogene, give to them their variable properties.

The colours of different specimens of phosphorus, as well as of sulphur, differ considerably; the red colour of phosphorus as it is commonly prepared, is probably owing to a slight mixture of oxide. Common roll sulphur is of a very pale yellow, the Sicilian sulphur of an orange colour, and the sulphur distilled from iron pyrites in vacuo, which arose in the last period of the process, of a pale yellowish green colour. All the late experiments that I have made, as well as my former researches, induce me to suspect a notable proportion of oxygene in Sicilian sulphur, which is probably owing to the presence of oxide of sulphur, which may give rise to sulphuric acid in distillation, or to sulphuric acid itself.

Conceiving that if definite proportions of oxygene and hydrogene existed in sulphur and phosphorus, they ought to be manifested in the agency of oxymuriatic acid gas on these bodies, I made some experiments on the results of these operations. In the first trial, on the combination of sulphur with oxymuriatic acid gas, I employed 5 grains of roll sulphur, and admitted the gas into the exhausted retort, from a vessel in which it had been in contact with warm water: in this case more than a half a cubical inch of oxygene gas, and nearly two cubical inches of muriatic acid gas, were produced. Suspecting in this instance, that aqueous vapour had been

decomposed, I employed cold water in the next experiment, and dried the gas by muriate of lime; in this case, though Sicilian sulphur was used, no oxygene gas was evolved; and not a half a cubical inch of muriatic acid; the quantity was the same as in the last experiment; and it was found, that between 16 and 17 cubical inches of oxymuriatic acid gas disappeared; the whole of the sulphur was sublimed in the gas, and the liquor formed, was of a tawney orange colour.

No oxygene was expelled during the combustion of phosphorus in oxymuriatic acid gas, nor could I ascertain that any muriatic acid had been formed; 8 grains of phosphorus were entirely converted into sublimate, by the absorption of about 23 cubical inches and a half of the gas.

It would seem from these quantities, that the sulphuretted liquor formed by subliming sulphur in oxymuriatic acid gas, consists of 1 proportion of sulphur, represented by 13.5, and 1 of oxymuriatic gas represented by 32.9, and that the phosphoric sublimate must be composed of 3 portions of oxymuriatic gas, represented by 98.7, and 1 of phosphorus represented by 16.5

XV. *Observations upon Luminous Animals.* *By* J. Macartney, *Esq. Communicated by* Everard Home, *Esq. F.R.S.*

Read May 17th, 1810.

THE property which certain animals possess of emitting light, is so curious and interesting, that it has attracted the attention of naturalists in all ages. It was particularly noticed by ARISTOTLE and PLINY amongst the ancients, and the publications of the different learned societies in Europe, contain numerous memoirs upon the subject. Notwithstanding the degree of regard bestowed upon the history of luminous animals, it is still very imperfect; the power of producing light appears to have been attributed to several creatures which do not possess it; some species which enjoy it in an eminent degree, have been imperfectly described or entirely unobserved; the organs which afford the light in certain animals have not been examined by dissection; and lastly, the explanations that have been given of the phænomena of animal light, are unsatisfactory, and in some instances palpably erroneous.

As this subject forms an interesting part of the history of organized beings, I have for some years availed myself of such opportunities as occurred for its investigation. Having communicated the result of some of my researches to the Right Honourable Sir JOSEPH BANKS, he immediately offered me his

assistance with that liberality, which so eminently distinguishes him as a real lover of science. I am indebted to him for an inspection of the valuable journal he kept during his voyage with Captain COOK; for permission to copy the original drawings in his possession, of those luminous animals discovered in both the voyages of COOK; and for some notes upon the luminous appearance of the sea, that were presented to him by Captain HORSBURG, whose accuracy of observation is already known to this learned Society.

In the following paper, I shall first examine the grounds on which the property of shewing light has been ascribed to certain animals, that either do not possess it, or in which its existence is questionable. I shall next give an account of some luminous species, of which some have been inaccurately described, and others quite unknown. I shall endeavour to explain from my own observations, and the information communicated to me by others, many of the circumstances attending the luminous appearance of the sea. I shall then describe the organs employed for the production of light in certain species; and, lastly, I shall review the opinions which have been entertained respecting the nature and origin of animal light, and relate the experiments I have made for the purpose of elucidating this part of the subject.

The property of emitting light has been reported to belong to several fishes, more particularly the mackarel, the moon-fish (tetraodon mola,) the dorado, mullet, sprat, &c.

Mr. BAJON observed during the migration of the dorados, &c. that their bodies were covered with luminous points. These however proved upon examination to be minute spherical particles that adhered to the surface of these fishes; and,

he adds, appeared to be precisely the same sort of points that illuminated the whole of the sea at the time. They were therefore in all probability the minute kind of medusa, which I shall have occasion to describe hereafter.

GODEHEU DE RIVILLE states, in a paper sent to the Academy of Sciences at Paris, that on opening the scomber pelamis while alive, he found in different parts of its body an oil which gave out much light: but it should be observed, that RIVILLE had a particular theory to support, for which this fact was very convenient, and that other parts of his memoir bear marks of inaccuracy. It may be added, that if the oil of fishes were usually luminous, which RIVILLE supposed, it would be almost universally known, instead of resting on a solitary observation.

As far as I am able to determine from what I have seen, the faculty of exhibiting light during life does not belong to the class of fishes. It appears probable, that some fishes may have acquired the character of being luminous, from evolving light soon after death.

Some species of lepas, murex, and chama, and some star-fish have been said to possess the power of shining; and the assertion has been repeated by one writer after another, but without quoting any authority.

BRUGUEIRE upon one occasion saw, as he supposed, common earth worms in a luminous state; all the hedges were filled with them; he remarked that the light resided principally in the posterior part of the body.*

FLAUGERGUES pretended to have seen earth worms luminous in three instances; it was at each time in October; the body

* Journal d'Histoire Naturelle, Tom. II.

shone at every part, but most brilliantly at the genital organs.*

Notwithstanding this concurrence of testimony, it is next to impossible, that animals so frequently before our eyes as the common earth worm, should be endowed with so remarkable a property, without every person having observed it. If they only enjoyed it during the season for copulation, still it could not have escaped notice, as these creatures are usually found joined together in the most frequented paths, and in garden walks.

In different systems of natural history, the property of shining is attributed to the cancer pulex. The authorities for this opinion are HABLITZL, and THULES and BERNARD. The former observed upon one occasion, a cable that was drawn up from the sea exhibit light, which upon closer inspection was perceived to be covered by these insects.† THULES and BERNARD reported that they met with a number of this species of cancer on the borders of a river, entirely luminous.‡ I am nevertheless disposed to question the luminous property of the cancer pulex, as I have often had the animal in my possession, and never perceived it emit any light.

The account given by LINNEUS of the scolopendra phosphorea, is so improbable and inconsistent, that one might be led to doubt this insect's existence, particularly as it does not appear to have been ever seen, except by EKEBERG, the Captain of an East Indiaman, from whom LINNEUS learnt its history.

* Journal de Physique, Tome XVI.

† Hablitzl ap. Pall. n. Nord. Beytr. 4, p. 396.

‡ Journal de Physique, Tome XXVIII.

I now proceed to the description of those luminous animals that have been discovered by the Right Honourable Sir JOSEPH BANKS, Captain HORSBURG, and myself.

On the passage from Madeira to Rio de Janeiro, the sea was observed by Sir JOSEPH BANKS to be unusually luminous, flashing in many parts like lightning. He directed some of the water to be hauled up, in which he discovered two kinds of animals that occasioned the phænomenon; the one, a crustaceous insect which he called the cancer fulgens; the other, a large species of medusa, to which he gave the name of pellucens.

The cancer fulgens bears some resemblance to the common shrimp; it is however considerably less, the legs are furnished with numerous setæ. The light of this animal, which is very brilliant, appears to issue from every part of the body.

The medusa pellucens measures about six inches across the crown or umbella; this part is marked by a number of opake lines, that pass off from the center to the circumference. The edge of the umbella is divided into lobules, which succeed each other, one large and two small ones alternately. From within the margin of the umbella, there are suspended a number of long cord-shaped tentacula. The central part of the animal is opake, and furnished with four thick irregularly shaped processes, which hang down in the midst of the tentacula.

This zoophyte is the most splendid of the luminous inhabitants of the ocean. The flashes of light emitted during its contractions, are so vivid as to affect the sight of the spectator.

In the notes communicated to Sir JOSEPH BANKS by Captain HORSBURG, he remarks that the luminous state of the sea

between the tropics, is generally accompanied with the appearance of a great number of marine animals of various kinds upon the surface of the water: to many of which he does not, however, attribute the property of shining. At other times, when the water which gave out light was examined, it appeared only to contain small particles of a dusky straw colour, which dissolved with the slightest touch of the finger. He likewise observes, that in Bombay, during the hot weather of May and June, he has frequently seen the edges of the sea much illuminated by minute sparkling points.

At sun rise on April 12, 1798, in the Arabian sea, he perceived several luminous spots in the water, which conceiving to be animals, he went in the boat and caught one. It proved to be an insect somewhat resembling in appearance the wood louse, and was about one third of an inch in length. When viewed with the microscope, it seemed to be formed by sections of a thin crustaceous substance. During the time that any fluid remained in the animal, it shone brilliantly like the fire fly.

In the month of June in the same year, he picked up another luminous insect on a sandy beach, which was also covered with a thin shell, but it was a different shape, and a larger size than the animal taken in the Arabian sea.

By comparing the above description with an elegant pen and ink drawing which was made by Captain Horsburg, and accompanied his paper, I have no doubt that both these insects were monoculi; the first evidently belongs to the genus limulus of Muller; I shall therefore beg leave to distinguish it by the name of limulus noctilucus.

My pursuits, and the state of my health, having frequently

led me to the coast, I have had many opportunities of making observations upon the animals which illuminate our own seas. Of these I have discovered three species: one of which is a beroe not hitherto described by authors; another agrees so nearly with the medusa hemispherica, that I conceive it to be the same, or at least a variety of that species; the third is a minute species of medusa, which I believe to be the luminous animal, so frequently seen by navigators, although it has never been distinctly examined or described.

I first met with these animals in the month of October 1804, at Herne Bay, a small watering place upon the northern coast of Kent. Having observed the sea to be extremely luminous for several nights, I had a considerable quantity of the water taken up. When perfectly at rest, no light was emitted, but on the slightest agitation of the vessel in which the water was contained, a brilliant scintillation was perceived, particularly towards the surface; and when the vessel was suddenly struck, a flash of light issued from the top of the water, in consequence of so many points shining at the same moment. When any of these sparkling points were removed from the water, they no longer yielded any light. They were so transparent, that in the air they appeared like globules of water. They were more minute than the head of the smallest pin. Upon the slightest touch, they broke and vanished from the sight. Having strained a quantity of the luminous water, a great number of these transparent corpuscles were obtained upon the cloth, and the water which had been strained, did not afterwards exhibit the least light. I then put some sea water that had been rendered particularly clear, by repeated filtrations, into a large glass, and having floated in it a fine cloth,

on which I had previously collected a number of luminous points, several of them were liberated, and became distinctly visible in their natural element, by placing the glass before a piece of dark coloured paper. They were observed to have a tendency to come to the surface of the water, and after the glass was set by for some time, they were found congregated together, and when thus collected in a body, they had a dusky straw colour, although individually they were so transparent, as to be perfectly invisible, except under particular circumstances. Their substance was indeed so extremely tender and delicate, that they did not become opaque in distilled vinegar or alcohol, until immersed in these liquors for a considerable time.

On examining these minute globules with the microscope, I found that they were not quite perfect spheres, but had an irregular depression on one side, which was formed of an opaque substance, that projected a little way inwards, producing such an appearance as would arise from tying the neck of a round bag, and turning it into the body.

The motions of these creatures in the water were slow and graceful, and not accompanied by any visible contraction of their bodies. After death they always subsided to the bottom of the vessel.

From the sparkling light afforded by this species, I shall distinguish it by the name of medusa scintillans.

The night following that, on which I discovered the preceding animal, I caught the two other luminous species. One of these I shall call the beroe fulgens.

This most elegant creature is of a colour changing between purple, violet, and pale blue; the body is truncated before,

and pointed behind; but the form is difficult to assign, as it is varied by partial contractions, at the animal's pleasure. I have represented the two extremes of form that I have seen this creature assume: the first is somewhat that of a cucumber, which as being the one it takes when at rest, should perhaps be considered as its proper shape: the other resembles a pear, and is the figure it has in the most contracted state. The body is hollow, or forms internally an infundibular cavity, which has a wide opening before, and appears also to have a small aperture, posteriorly through which it discharges its excrement. The posterior two-thirds of the body are ornamented with eight longitudinal ciliated ribs, the processes of which are kept in such a rapid rotatory motion, while the animal is swimming, that they appear like the continual passage of a fluid along the ribs. The ciliated ribs have been described by Professor MITCHELL, as arteries, in a luminous beroe, which I suspect was no other than the species I am now giving an account of.

When the beroe fulgens swam gently near the surface of the water, its whole body became occasionally illuminated in a slight degree; during its contractions, a stronger light issued from the ribs, and when a sudden shock was communicated to the water, in which several of these animals were placed, a vivid flash was thrown out. If the body were broken, the fragments continued luminous for some seconds, and being rubbed on the hand, left a light like that of phosphorus: this however, as well as every other mode of emitting light, ceased after the death of the animal.

The hemispherical species that I discovered, had a very faint purple colour. The largest that I found, measured about

three quarters of an inch in diameter. The margin of the umbella was undivided, and surrounded internally by a row of pale brown spots, and numerous small twisted tentacula: four opaque lines crossed in an arched manner from the circumference, towards the center of the animal; an opaque irregular shaped process hung down from the middle of the umbella: when this part was examined with a lens of high powers, I discovered that it was inclosed in a sheath in which it moved, and that the extremity of the process was divided into four tentacula, covered with little cups or suckers, like those on the tentacula of the cuttle fish.

This species of medusa bears a striking resemblance to the figures of the medusa hemispherica, published by GRONOVIUS and MULLER; indeed it differs as little from these figures, as they do from each other. Its luminous property, however, was not observed by these naturalists, which is the more extraordinary, as MULLER examined it at night, and says it is so transparent, that it can only be seen with the light of a lamp. If it should be still considered as a distinct species, or as a variety of the hemispherica, I would propose to call it the medusa lucida.

In this species, the central part and the spot round the margin, are commonly seen to shine on lifting the animal out of the water into the air, presenting the appearance of an illuminated wheel, and when it is exposed to the usual percussion of the water, the transparent parts of its body are alone luminous.

In the month of September 1805, I again visited Herne Bay, and frequently had opportunities of witnessing the luminous appearance of the sea. I caught many of the hemi-

spherical and minute species of medusa, but not one of the beroe fulgens. I observed that these luminous animals always retreated from the surface of the water, as soon as the moon rose. I found also, that exposure to the day light took away their property of shining, which was revived by placing them for some time in a dark situation.

In that season I had two opportunities of seeing an extended illumination of the sea, produced by the above animals. The first night I saw this singular phenomenon, was extremely dark, many of the medusa scintillans, and medusa hemispherica had been observed at low water, but on the return of the tide, they had suddenly disappeared. On looking towards the sea, I was astonished to perceive a flash of light of about six yards broad, extend from the shore, for apparently the distance of a mile and a half along the surface of the water. The second time that I saw this sort of light proceed from the sea, it did not take the same form, but was diffused over the surface of the waves next the shore, and was so strong, that I could for the moment distinctly see my servant, who stood at a little distance from me; he also perceived it, and called out to me at the same instant. On both these occasions the flash was visible for about four or five seconds, and although I watched for it a considerable time, I did not see it repeated.

A diffused luminous appearance of the sea, in some respects different from what I have seen, has been described by several navigators.

GODEHEU DE RIVILLE saw the sea assume the appearance a plain of snow on the coast of Malabar.*

* Mem. Etrang. de l' Acad. des Sc. Tom. 3.

Captain HORSBURGH, in the notes he gave to Sir JOSEPH BANKS, says, there is a peculiar phenomenon sometimes seen within a few degrees distance of the coast of Malabar, during the rainy monsoon, which he had an opportunity of observing. At midnight the weather was cloudy, and the sea was particularly dark, when suddenly it changed to a white flaming colour all around. This bore no resemblance to the sparkling or glowing appearance he had observed on other occasions in seas near the equator, but was a regular white colour, like milk, and did not continue more than ten minutes. A similar phenomenon, he says, is frequently seen in the Banda sea, and is very alarming to those who have never perceived or heard of such an appearance before.

This singular phenomenon appears to be explained by some observations communicated to me by Mr. LANGSTAFF, a surgeon in the city, who formerly made several voyages. In going from New Holland to China, about half an hour after sunset, every person on board was astonished by a milky appearance of the sea: the ship seemed to be surrounded by ice covered with snow. Some of the company supposed they were in soundings, and that coral bottom gave this curious reflection, but on sounding with 70 fathoms of line no bottom was met with. A bucket of water being hauled up, Mr. LANGSTAFF examined it in the dark, and discovered a great number of globular bodies, each about the size of a pins head, linked together. The chains thus formed did not exceed three inches in length, and emitted a pale phosphoric light. By introducing his hand into the water, Mr. LANGSTAFF raised upon it several chains of the luminous globules, which were separated by opening the fingers, but readily re-united

on being brought again into contact, like globules of quicksilver. The globules, he says, were so transparent, that they could not be perceived when the hand was taken into the light.

This extraordinary appearance of the sea was visible for two nights. As soon as the moon exerted her influence, the sea changed to its natural dark colour, and exhibited distinct glittering points, as at other times. The phenomenon, he says, had never been witnessed before by any of the company on board, although some of the crew had been two or three times round the globe.

I consider this account of Mr. LANGSTAFF very interesting and important, as it proves that the diffused light of the sea is produced by an assemblage of minute medusæ on the surface of the water.

In June 1806, I found the sea at Margate more richly stored with the small luminous medusæ, than I have ever seen it. A bucket of the water being set by for some time, the animals sought the surface, and kept up a continual sparkling, which must have been occasioned by the motions of individuals, as the water was perfectly at rest. A small quantity of the luminous water was put into a glass jar, and on standing some time, the medusæ collected at the top of the jar, and formed a gelatinous mass, one inch and a half thick, and of a reddish or mud colour, leaving the water underneath perfectly clear.

In order to ascertain if these animals would materially alter their size, or assume the figure of any other known species of medusa, I kept them alive for 25 days, by carefully changing the water in which they were placed; during which time,

although they appeared as vigorous as when first taken, their form was not in the slightest degree altered, and their size but little increased. By this experiment I was confirmed in the opinion of their being a distinct species, as the young actiniæ and medusæ exhibit the form of the parent in a much shorter period than the above.

In September 1806, I took at Sandgate a number of the beroe fulgens, but no other species: they were of various dimensions, from the full size down to that of the medusa scintillans: they could however be clearly distinguished from the latter species, by their figure.

Since that time, I have frequently met with the medusa scintillans on different parts of the coast of Sussex, at Tenby, and at Milford haven. I have likewise seen this species in the bays of Dublin and Carlingford in Ireland.

In the month of April, last year, I caught a number of the beroe fulgens in the sea at Hastings; they were of various sizes, from about the half of an inch in length, to the bulk of the head of a large pin. I found many of them adhering together in the sea; some of the larger sort were covered with small ones, which fell off when the animals were handled, and by a person unaccustomed to observe these creatures, would have been taken for a phosphoric substance. On putting a number of them into a glass, containing clear sea water, they still shewed a disposition to congregate upon the surface. I observed that when they adhered together, they shewed no contractile motion in any part of their body, which explains the cause of the pale or white colour of the diffused light of the ocean. The flashes of light which I saw come from the sea at Herne bay, were probably produced by a

sudden and general effort of the medusæ to separate from each other, and descend in the water.

The medusa scintillans almost constantly exists in the different branches of Milford haven that are called pills. I have sometimes found these animals collected in such vast numbers in those situations, that they bore a considerable proportion to the volume of the water in which they were contained: thus, from a gallon of sea water in a luminous state, I have strained above a pint of these medusæ. I have found the sea under such circumstances to yield me more support in swimming, and the water to taste more disagreeably than usual; probably the difference of density, that has been remarked at different times in the water of the sea, may be referred to this cause.

All my own observations lead me to conclude, that the medusa scintillans, is the most frequent source of the light of the sea around this country, and by comparing the accounts of others with each other, and with what I have myself seen, I am persuaded that it is so likewise in other parts of the world. Many observers appear to have mistaken this species for the nereis noctiluca, which was very natural, as they were prepossessed with the idea of the frequent existence of the one, and had no knowledge of the other. Some navigators have actually described this species of medusa, without being aware of its nature. Mr. BAJON, during his voyage from France to Cayenne, collected many luminous points in the sea, which he says, when examined by a lens, were found to be minute spheres. They disappeared in the air. Doctor LE ROY, in sailing from Naples to France, observed the sparkling appearance of the sea, which is usually produced by the medusa scintillans.

By filtering the water, he separated luminous particles from it, which he preserved in spirits of wine: they were, he says, like the head of a pin, and did not at all resemble the nereis noctiluca, described by VIANELLI; their colour approached a yellow brown, and their substance was extremely tender, and fragile. Notwithstanding this striking resemblance to the medusa scintillans, LE ROY, in consequence of a preconceived theory, did not suppose what he saw, were animals, but particles of an oily or bituminous nature.*

The minute globules seen by Mr. LANGSTAFF in the Indian ocean, were I think, in all probability, the scintillating species of medusa; and on my shewing him some of these animals I have preserved in spirits, he entertained the same opinion.

Professor MITCHELL, of New York, found the luminous appearance on the coast of America, to be occasioned by minute animals, that from his description, plainly belonged to this species of medusa, notwithstanding which, he supposed them to be a number of the nereis noctiluca.†

The luminous animalcule, discovered by FORSTER off the Cape of Good Hope, in his voyage round the world, bears so strong a resemblance to the medusa scintillans, that I am much disposed to believe them the same. He describes his animalcule as being a little gelatinous globule, less than the head of a pin; transparent, but a little brownish in its colour; and of so soft a texture, that it was destroyed by the slightest touch. On being highly magnified, he perceived on one side a depression, in which there was a tube that passed into the

* Observ. sur un lumiere produite par L'Eau de la mer. Mem. Etrang. des Sc.

† Phil. Mag, Vol. X. p. 20.

body, and communicated with four or five intestinal sacs. The pencil drawings he made on the spot, are in the possession of Sir JOSEPH BANKS, by whose permission, engravings from them are subjoined to this paper. By comparing these with the representations of the medusa scintillans, and some of this species rendered visible, by being a long time preserved in spirits, which I have laid before this learned Society, it will be found, that the only difference between FORSTER's animalcule, and the medusa scintillans, is in the appearance of the opaque parts, shewn in the microscopic views.

Many writers have ascribed the light of the sea to other causes than luminous animals. MARTIN supposed it to be occasioned by putrefaction: SILBERSCHLAG believed it to be phosphoric: Professor J. MAYER conjectured, that the surface of the sea imbibed light, which it afterwards discharged. BAJON and GENTIL thought the light of the sea was electric, because it was excited by friction. FORSTER conceived that it was sometimes electric, sometimes caused from putrefaction, and at others by the presence of living animals. FOUGEROUX DE BONDAROY believed that it came sometimes from electric fires, but more frequently from the putrefaction of marine animals and plants.

I shall not trespass on the time of the Society, to refute the above speculations; their authors have left them unsupported by either arguments or experiments, and they are inconsistent with all ascertained facts upon this subject.

The remarkable property of emitting light during life, is only met with amongst animals of the four last classes of modern naturalists, viz. MOLLUSCA, INSECTS, WORMS, and ZOOPHYTES.

The mollusca and worms contain each but a single luminous species; the *pholas dactylus* in the one, and the *nereis noctiluca* in the other.

Some species yield light, in the eight following genera of insects; *elater, lampyris, fulgora, pausus, scolopendra, cancer, lynceus,** and *limulus*. The luminous species of the genera lampyris, and fulgora, are more numerous than is generally supposed, if we may judge from the appearance of luminous organs, to be seen in dried specimens.

Amongst zoophytes we find that the genera *medusa, beroe,*† and *pennatula*, contain species which afford light.

The only animals which appear to possess a distinct organization for the production of light, are the luminous species of lampyris, elater, fulgora, and pausus.

The light of the lampyrides is known to proceed from some of the last rings of the abdomen, which when not illuminated, are of a pale yellow colour. Upon the internal surface of these rings, there is spread a layer of a peculiar soft yellow substance, which has been compared to paste, but by examination with a lens, I found it to be organized like the common interstitial substance of the insect's body, except that it is of a closer texture, and a paler yellow colour. This substance does not entirely cover the inner surface of the rings, being more or less deficient along their edges, where it presents an irregular waving outline. I have observed in the glow worm, that it is absorbed, and its place supplied by

* The animal discovered by Riville off the coast of Malabar in 1754, is certainly a testaceous insect, and appears to belong to the genus lynceus of Muller.

† The luminous zoophyte for which Peron has lately instituted the new genus pytosoma, appears to me to be a beroe, and only worthy of a specific distinction.

common interstitial substance, after the season for giving light is past.

The segments of the abdomen, behind which this peculiar substance is situated, are thin and transparent, in order to expose the internal illumination.

The number of luminous rings varies in different species of lampyris, and as it would seem at different periods in the same individual.

Besides the luminous substance above described, I have discovered in the common glow worm, on the inner side of the last abdominal ring, two bodies, which to the naked eye appear more minute than the head of the smallest pin. They are lodged in two slight depressions, formed in the shell of the ring, which is at these points particularly transparent. On examining these bodies under the microscope, I found that they were sacs containing a soft yellow substance, of a more close and homogeneous texture, than that which lines the inner surface of the rings. The membrane forming the sacs, appeared to be of two layers, each of which is composed by a transparent silvery fibre, in the same manner as the internal membrane of the respiratory tubes of insects, except that in this case, the fibre passes in a spiral, instead of a circular direction. This membrane, although so delicately constructed, is so elastic as to preserve its form, after the sac is ruptured, and the contents discharged.

The light that proceeds from these sacs, is less under the controul of the insect, than that of the luminous substance spread on the rings: it is rarely ever entirely extinguished in the season that the glow worm gives light, even during the day; and when all the other rings are dark, these sacs often shine brightly.

The circumstance of there being points, which give a more permanent light than the other parts of the luminous rings of the abdomen, has been noticed before by the Comte G. DE RAZOUMOUSKI. He states the number of these luminous points to vary from 2 to 5.*

I must however remark, that I never saw more than two of these luminous points, which were always upon the last ring of the body, and that the figures which accompany the memoir of the Comte DE RAZOUMOUSKI, bear scarcely any resemblance to the insect they are intended to represent, from which we may fairly suspect him of inaccuracy in other particulars.

As far as my observation has extended, the small sacs of luminous substances are not found in any species of lampyris, except the glow worm of this country. THUNBURG mentions that the lampyris japonica has two vesicles on the tail, which afford light.

The organs for the production of light in the genus elater, are situated in the corcelet; these likewise consist of a peculiar yellow substance, placed behind transparent parts of the shell, which suffer the natural colour of this substance to be seen through them in the day, and when illuminated, give passage to the light.

On dissecting the organs of light in the elater noctilucus, I found that there is a soft yellow substance, of an oval figure, lodged in the concavity of the yellow spots of the corcelet, which parts are particularly thin and transparent in this species. This substance is so remarkably close in its structure, that at first view it appears like an inorganic mass, but with

* Mem. de la Soc. de Lausanne, Tom. ii.

a lens, it is readily perceived to be composed of a great number of very minute parts or lobules closely pressed together. Around these oval masses, the interstitial substance of the corcelet is arranged in a radiated manner, and the portion of the shell that immediately covers the irradiated substance, is in a certain degree transparent, but less so than that which lies over the oval masses, it is therefore probable, that the interstitial substance in this situation, may be endowed with the property of shining. A fasciculus of the muscles of the corcelet arises in the interior of the oval masses of the luminous substance, but not apparently with any design, as it contributes, with the adjacent fasciculi, to move the anterior feet.

In the elater ignitus, the masses of luminous substance are extremely irregular in their figure: they are situated nearly at the posterior angles of the corcelet, and are more loose in their texture than the oval masses of the noctilucus, resembling rather in composition, the interstitial substance which surrounds these masses in that species. The shell of the corcelet is somewhat thinner, and more transparent along both sides of the margin, than at other places, but it is not, as in the noctilucus, elevated, and peculiarly clear and thin immediately, over the seat of the luminous organ; consequently, the light emitted by the elater ignitus, cannot be very brilliant.

I have not been able to procure any specimen of the elater phosphorea, but from the accounts of naturalists, it appears to resemble in every respect the elater noctilucus; indeed I have great doubts of the phosphorea being a distinct species.

I have had an opportunity of examining, preserved in a moist way, two species of fulgora, the candelaria and lanter-

naria. The light in this genus has been observed to issue from the remarkable proboscis on the fore part of the head. This part has always been described by authors, as hollow or empty, which I have found to be perfectly correct; and what is more extraordinary, that the cavity communicates freely with the external air, by means of a chink or narrow aperture, placed on each side of the root of the proboscis. This projection is covered internally by a membrane, between which and the horny part or shell, there appears to be interposed a pale reddish coloured soft substance, that is arranged in the candelaria in broad lines or stripes; but it is so thin, that I could not distinctly examine its structure, or absolutely determine, whether it should be considered as a substance intended to furnish the light of these insects, or the pigment upon which the colour of the proboscis depends.

The globes of the antennæ constitute the organs of light in the pausus spherocerus. Dr. Afzelius, who discovered the luminous property in this species, compares them to lanterns spreading a dim phosphoric light.* The rarity of the insect put it out of my power to examine its structure, but from the form and situation of its organs of light, it is most probable they are constructed like those of the fulgoræ.

It has been conjectured by Carradori and others, that the lampyrides were enabled to moderate or extinguish their light, by retracting the luminous substance under a membrane; but neither in them, or any of the other luminous insects, have I found an apparatus of this sort. The substance furnishing the light, is uniformly applied to corresponding transparent parts of the shell of the insect from whence it is not moved; indeed a

* Lin. Trans. Vol. IV.

membrane, if it did exist, would have but little effect in obscuring the light, and never could serve to extinguish it. The regulation of the kind and degree of the luminous appearance, does not depend upon any visible mechanism, but like the production of the light itself, is accomplished by some inscrutable change in the luminous matter, which in some animals is a simple operation of organic life, and in others is subject to the will.

It is worthy of remark, that in all the dissections I have made of luminous insects, I did not find that the organs of light were better, or differently supplied with either nerves or air tubes, than the other parts of the body. The power of emitting light likewise exists in many creatures which want nerves, a circumstance strongly marking a difference between animal light, and animal electricity.

With the exception of the animals above mentioned, the exhibition of light depends upon the presence of a fluid matter.

In the pholas dactylus, the luminous fluid is particularly evident, and in vast quantity; it is recorded by PLINY, that this fluid is like liquid phosphorus, and renders every object luminous with which it comes into contact. REAUMUR also found that it was diffusible in water, or any other fluid in which the animal might be immersed.*

The shining of the scolopendra electrica, I have always observed to be accompanied by the appearance of an effusion of a luminous fluid upon the surface of the animal, more particularly about the head, which may be received upon the hand, or other bodies brought into contact with the insect at the

* Mem. de l' Acad. des Sc. 1712.

moment, and these exhibit a phosphoric light for a few seconds afterwards. This fluid, however, I never could discover in the form of moisture, even upon the clearest glass, although examined immediately with the most scrupulous attention by a lens: it must therefore be extremely attenuated.

The same appearance has been observed during the illumination of the nereis noctiluca by Fougeroux de Bondaroy.*

The animal discovered by Riville shed a blue liquor, which illuminated the water for a distance of two or three lines.†

Spallanzani relates, that the medusa which he examined, communicated the property of shining to water, milk, and other fluids, on being rubbed or squeezed in them.‡

The luminous fluid is in some instances confined to particular parts of the body, and in others is diffused throughout the whole substance of the animal.

In the scolopendra electrica, it appears to reside immediately under the integuments. In the lynceus discovered by Riville, it is contained in the ovary. If I may judge from my own observations, every part of the body of the medusæ is furnished with this fluid, as there is no part I have not seen illuminated under different circumstances, but Spallanzani affirms that it is only found in the large tentacula, the edges of the umbella, and the purse or central mass; which he proved, he says, by detaching these parts successively, when they shone vividly, while the rest of the body neither gave light or communicated any luminous appearance to water.§

* Mem. de l' Acad. des Sc. 1767.

† Mem. Etrang. de l' Acad. des Sc. Tom. iii.

‡ Spallanzani's Travels in the Two Sicilies, Vol. iv.

§ Memoria sopra le meduse fosforiche. Mem. della Soc. Ital. Tomo vii.

SPALLANZANI discovered a mucous luminous fluid in the plumule of the pennatula phosphorea.*

The phenomenon of animal light has been attempted to be explained in different ways. By many persons it was formerly ascribed to a putrefactive process, but since the modern theories of combustion became known, it has been generally believed to depend upon an actual inflammation of the luminous substance, similar to the slow combustion of phosphorus. Others have accounted for the luminous effect, by supposing the matter of light to be accumulated, and rendered latent under particular circumstances, and afterwards evolved in a sensible form.

The opinion of the light of living animals being the consequence of putrefaction, is evidently absurd, and contradictory to all observation on the subject. It has been proved by the experiments of Dr. HULME and others, that even the luminous appearances of dead animals, are exhibited only during the first stages of the dissolution of the body, and that no light is emitted after putrefaction has really commenced.

SPALLANZANI, who was the most strenuous advocate for the phosphorescent nature of animal light, stated that glow-worms shone more brilliantly when put into oxygen gas; that their light gradually disappeared in hydrogen or in azotic gas, and was instantly extinguished in fixed air; that it was also lost by cold, and revived by the application of a warm temperature. He conjectured that the luminous matter of these insects was composed of hydrogen and carbonated hydrogen gas.

FORSTER relates, in the Lichtenberg Magazine for 1783,

* Mem. della Soc. Ital. Tomo ii.

that on putting a lampyris splendidula into oxygen gas, it gave as much light as four of the same species in common air.

Carradori has made some experiments upon the lucciole, (lampyris italica) which led him to deny its phosphorescence. He found that the luminous portion of the belly of the insect shone in vacuum, in oil, in water, and different liquids, and under different circumstances, where it was excluded from all communication with oxygen gas. He accounts for the result of Forster's experiment, by supposing, that the worm shone more vividly, because it was more animated in oxygen gas than in common air.

Carradori adopts on this subject the doctrine of Brugnatelli, and ascribes the luminous appearances of animals, to the condensation and extrication of light in particular organs, which had previously existed in combination with the substance of their bodies. He supposes the light to be originally derived from the food, or the atmospheric air taken into the body; in short, that certain animals have the peculiar property of gradually imbibing light from foreign bodies, and of afterwards secreting it in a sensible form.*

The following experiments which I made upon this subject, would lead me to make different conclusions than those of the preceding authors.

Experiment 1.—A glow worm was put into a glass of water, in which it lived nearly two hours, and continued to emit light as usual, until it died, when the luminous appearance entirely ceased.

Experiment 2.—The luminous substance was extracted

* Annal di Chimica, Tomo xii. 1797.

from the beforementioned glow-worm, and from others killed in different ways, but it afforded no light.

Experiment 3.—The sacs containing the luminous matter were cut from the bellies of *living* glow-worms, and shone uninterruptedly for several hours in the atmosphere, and after their light became extinct, it was revived by being moistened with water; some of these were put into water in the first instance, in which they continued to shine unremittingly for 48 hours.

Experiment 4.—The luminous substance of a glow-worm was exposed to a degree of heat which would have been sufficient to inflame phosphorus, without increasing the brilliancy of its light; and farther, it could not be made to burn by being applied to a red hot iron, or to the flame of a candle.

Experiment 5.—A delicate thermometer was introduced amongst some living glow-worms, during the time they gave out much light: the temperature of the room being 69, the instrument rose to 75, 76, and 77, according to circumstances, as the warmth was reflected from the hand, or dissipated by the worm crawling over cold substances. The luminous portion of the tail, when very brilliant, appeared to raise the thermometer more quickly than the other parts of the body, but it was not invariably the case. When shining strongly, I thought that the luminous rings communicated the sensation of warmth to the hand, but this was probably a deception, as the actual degree of heat was not sufficient for such an effect. It should however be mentioned, that in TEMPLAR's observations on the glow-worm, he said his feelings deceived him, if he did not experience some heat from the shining of the insect.*

* Phil. Trans. No. 72.

Experiment 6.—To satisfy myself how far the evolution of heat during the shining of glow-worms, depended upon the life of the animals, I cut off the luminous portion of the tail from several living worms, and I found that if the thermometer was applied to them immediately, it was raised by them one or two degrees; but after these parts were dead, although they continued to emit light, they produced no effect whatever upon the instrument.

Experiment 7.—Some hemispherical medusæ were put into a spoon, containing a small quantity of sea water, and held over a burning candle. As soon as the water became heated the medusæ appeared like illuminated wheels, the spots at the margin and center alone emitting light; in which manner they shone vividly and permanently for about 20 seconds, when they shrunk and died, after which they were no longer luminous.

Experiment 8.—Some of the same species were put into spirits: a strong and unremitting light was instantly given out, which issued from the central and marginal parts, as in the preceding experiment, and continued until they died.

Experiment 9.—Some of the scintillating and hemispherical species of medusa, contained in a small glass jar, were introduced into the receiver of an air pump, and the air being exhausted, they shone as usual when shaken; if any difference could be perceived, the light was more easily excited, and continued longer in vacuum.

I wished next to try the influence of electricity on the luminous property of animals.

Experiment 10.—A medusa hemispherica was placed in a small glass dish, containing a quantity of water, merely

sufficient to allow the animal to preserve its figure; being insulated, it was electrified, and sparks drawn from it, which had not the slightest effect; the experiment was repeated several times with different individuals, but without exciting the animals to throw out light.

Experiment 11.—Some hemispherical medusæ were placed in contact with the two ends of an interrupted chain, and slight electric shocks passed through them. During the very moment of their receiving the shock no light was visible, but immediately afterwards the medusæ shone like illuminated wheels, which appearance remained for some seconds. Upon the closest inspection with a magnifying glass, no contractile motion could be perceived to accompany the exhibition of the light. The application of electricity in this instance seems to have acted merely as a strong mechanic shock.

The above experiments on the luminous medusæ were made at Herne, with the assistance of GEORGE MAY, Esq. of Stroud-house, and in the presence of a large company, capable of accurately distinguishing their results.

It seems proved by the foregoing experiments, that so far from the luminous substance being of a phosphorescent nature, it sometimes shews the strongest and most constant light, when excluded from oxygene gas; that it in no circumstances undergoes any process like combustion, but is actually incapable of being inflamed; that the increase of heat, during the shining of glow-worms, is an accompaniment, and not an effect of the phenomenon, and depends upon the excited state of the insect; and lastly, that heat and electricity increase the exhibition of light, merely by operating like other stimuli upon the vital properties of the animal.

In confirmation of these opinions, I may quote the high authority of the Secretary of this Society, who has found that the light of the glow-worm is not rendered more brilliant in oxygene, or in oxygenated muriatic gas, than in common air; and that it is not sensibly diminished in hydrogene gas.

I may further add, that SPALLANZANI's experiments of diffusing the luminous liquor of the medusa in water, milk, and other fluids, are in direct contradiction of his own theory, as is also the extinction of the light of these mixtures by the application of a high degree of heat.

If the light emitted by animals were derived from their food, or the air they respire, as supposed by CARRADORI, the phenomenon should be increased or diminished, according to the quantity of food or air, that the creatures consume; but we do not find this to be the case; for in those situations where they are sometimes found to be most luminous, they are deprived, in a great measure, of these assumed sources of their light.

In fact, the luminous exhibitions of living animals are not only independent of all foreign light, but are frequently destroyed by the latter. I have always found the shining of the medusæ to cease upon the rising of the moon, or at the approach of day; and when out of the sea, I never could excite them to throw out light until they had been kept for some time in the dark; all the luminous insects likewise secrete themselves as much as possible during the day time, and go abroad only at night. I have, it is true, found that the scolopendra electrica will not shine unless it has been previously exposed to solar light; but I have observed that it shone as brilliantly and as frequently, after being kept a short time in a light

situation, as when left uncovered the whole day. The circumstance of the scolopendra requiring exposure previous to its giving out light, is very unaccountable, as the insect, when left to itself, always seeks as much as possible concealment during the day; indeed it is the opinion of some naturalists that it is killed by the light of the sun.

The opinions of BRUGNATELLI and CARRADORI are connected with some general doctrines, respecting the nature of light, which I shall not at present venture to discuss. It appears to me, that the question is still unresolved, whether light has a substantial existence, or is a phenomenon depending upon certain operations or conditions of the ordinary forms of matter. But the highly ingenious researches of Count RUMFORD, on the laws of what have been called subtile fluids, and the extraordinary advances lately made by Mr. DAVY, on the decomposition of substances, that were hitherto looked upon as elementary, give us reason to hope, that future investigations may unfold views of the material world, of which we can at present have only an indistinct conception; that new modes of analysis may enable us to see things, not "through a glass darkly," but more nearly as they are; and that the boundaries of physical and metaphysical science, now so far asunder, may be made to approach each other.

In the present state of our knowledge, our business should be, to collect, arrange, and compare phenomena, rather than to speculate upon their nature. Nevertheless, I cannot refrain from observing, that the circumstances attending the luminous appearance of living animals, are much more favourable to the supposition of light being a property, than a substance. The quantity of light emitted by an animal in a certain time,

(admitting it to be matter) far exceeds that which could be possibly supplied by the sources, from whence it is usually supposed to be derived. Thus the luminous appearance of some medusæ may be continued with the intermission of short intervals for an indefinite time, notwithstanding the creature be kept in darkness, and without any other food than what a small quantity of filtered sea water would afford. The uninterrupted and long continued light that is sometimes evolved by the luminous sacs, and the ova of the glow-worm, is also inconsistent with the notion of an accumulation and subsequent dispersion of a material substance.

I shall terminate this paper by an enumeration of the several conclusions, that are the result of the observations I have been able to make upon the phenomena of animal light.

The property of emitting light is confined to animals of the simplest organization, the greater number of which are inhabitants of the sea.—The luminous property is not constant, but in general, exists only at certain periods, and in particular states of the animal's body.—The power of shewing light, resides in a peculiar substance or fluid, which is sometimes situated in a particular organ, and at others diffused throughout the animal's body.—The light is differently regulated, when the luminous matter exists in the living body, and when it is abstracted from it. In the first case, it is intermitting, or alternated with periods of darkness; is commonly produced or increased by a muscular effort; and is sometimes absolutely dependant upon the will of the animal. In the second case, the luminous appearance is usually permanent until it becomes extinct, after which it may be restored directly by friction, concussion, and the application of warmth; which last causes,

operate on the luminous matter (while in the living body,) only indirectly, by exciting the animal.—The luminous matter, in all situations, so far from possessing phosphoric properties, is incombustible, and loses the quality of emitting light, by being dried, or much heated.—The exhibition of light, however long it may be continued, causes no diminution of the bulk of the luminous matter. It does not require the presence of pure air, and is not extinguished by other gasses.

The luminous appearance of living animals is not exhausted by long continuance, or frequent repetitions, nor accumulated by exposure to natural light; it is therefore, not dependent upon any foreign source, but inheres as a property, in a peculiarly organized animal substance or fluid, and is regulated by the same laws which govern all the other functions of living beings.

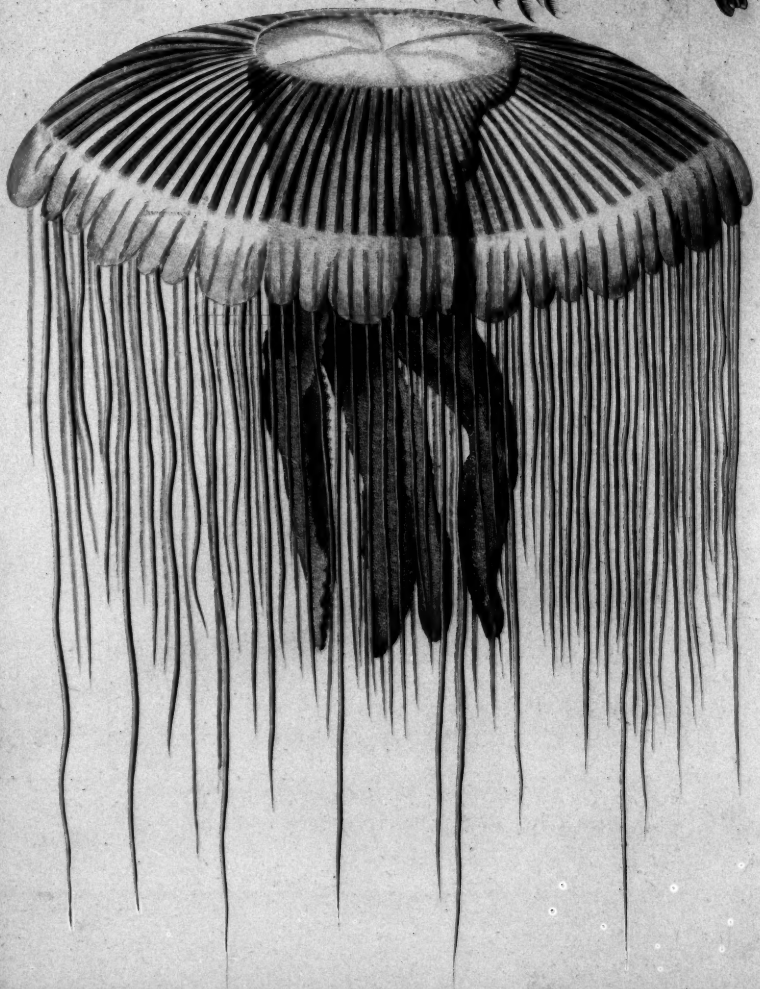
The light of the sea is always produced by living animals, and most frequently by the presence of the medusa scintillans. When great numbers of this species approach the surface, they sometimes coalesce together, and cause that snowy or milky appearance of the sea, which is so alarming to navigators. These animals, when congregated on the surface of the water, can produce a flash of light, somewhat like an electric corruscation. When the luminous medusæ are very numerous, as frequently happens in confined bays, they form a considerable portion of the mass of the sea, at which times they render the water heavier, and more nauseous to the taste; it is therefore adviseable to always strain sea water before it is drunk.

The luminous property does not appear to have any connection with the œconomy of the animals that possess it,

Fig. 1.

Fig. 2.

Fig. 3.



except in the flying insects, which by that means discover each other at night, for the purpose of sexual congress.

Explanation of the Figures.

Fig. 1. The cancer fulgens, discovered by the Right Hon. Sir Joseph Banks, of the natural size.

Fig. 2. The same animal magnified.

Fig. 3. The medusa pellucens, also found by Sir Joseph Banks, represented of the natural magnitude.

Fig. 4. The limulus noctilucus, discovered by Captain Horsburgh, considerably enlarged.

Fig. 5. The luminous medusa, discovered by me, which I conceive to be the medusa hemispherica: it is shewn of the largest size I met with.

Fig. 6. The central process of this animal's body magnified, in order to explain its structure. The thick tentacula in which it terminates are seen covered with small cups or suckers.

Fig. 7. The beroe fulgens, discovered by me, shewn in its most elongated or relaxed form, which it assumes commonly when swimming quickly.

Fig. 8. The same animal in the most contracted form.

Fig. 9. The minute species of medusa, discovered by me, which is the most frequent cause of the luminous appearance of the sea, represented of the natural size.

Fig. 10. The same animal magnified, exhibiting a puckered or tucked-in appearance on one side.

Fig. 11. Is the animalcule discovered by Forster, of the natural size.

Fig. 12. The same, greatly magnified, to shew the intes-

tinal parts. Both these figures are copied from the original drawings, in the possession of the Right Hon. Sir JOSEPH BANKS.

Fig. 13. Is an enlarged view of the inferior surface of the abdomen of the lampyris lucida, after the integument had been removed. *a a a* represent the three masses of luminous substance which are applied to the three last rings of the abdomen. *b b b* the arrangement of cellular or interstitial substance on the other abdominal rings, which gives the pale colour to the whole belly of this insect.

Fig. 14. Represents the common glow-worm, with the posterior portion of the back cut away to expose the sacs of luminous matter in situ on the last ring of the belly. *a* indicates the sac of one side; the intestine is seen to lie between them.

Fig. 15 and 16. Are the sacs of the glow-worm prodigiously magnified to shew their structure. Fig. 16 is cut open to expose the luminous matter it contains: the coat of the sac is still seen to preserve its figure.

Fig. 17. Is the elater noctilucus, with the shell of the corcelet removed on one side, by which the organ of light is uncovered. *a* the yellow transparent spot of the corcelet. *b* the oval mass of luminous substance surrounded by an irradiation of the interstitial substance. *c* the ends of the muscles which were on the inside of the corcelet.

Fig. 18. Is the posterior angle of the corcelet of the elater noctilucus magnified. *a* the radiated appearance which the interstitial substance has round the oval mass of luminous matter. This mass is seen to consist of a number of smaller parts. *b* shews the appearance of the interstitial substance,

Fig. 4

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J. Basire sc.

where it passes down between the muscles. *c* the ends of the muscles of the back. *d* the shell of the corcelet.

Fig. 19. Represents the elater ignitus. *a* is the mass of luminous substance of one side, seen indistinctly through the back part of the semitransparent portion of the corcelet. *b* is the luminous mass of the other side, exposed by removing a part of the shell of the corcelet.

XVI. *Observations and Experiments on Pus.* By George Pearson, *M.D. F.R.S.*

Read July 5th, 1810.

Chemical writers vary in their statements of the properties of pus; and they consider that a further investigation is requisite for the purposes of science. Physicians confess that, in numerous cases, they cannot form a satisfactory judgment of the nature of diseases, on account of not being able to determine what is, and what is not purulent matter; likewise probably, on account of the existence of different kinds, or varieties, at least, of this substance, afforded by different disorders.

I beg leave, therefore, to submit to this learned Society, my own observations, experiments, and reasoning on this animal matter.

Section I. *Simple, and obvious Properties.*

The different kinds of fluid, commonly considered to be pus, may be distinguished by the following titles:

I. The cream-like and equally consistent.

II. The curdy and unequal in consistence.

III. The serous and thin kind.

IV. The thick, viscid or slimy.

I. A pint of the first sort was taken out of the pericardium, after a fatal inflammation of the heart, in St. George's Hospital, and obligingly sent to me by my colleague, Dr. E. N. Bancroft.

The *colour* was yellowish—the *smell* was fleshy when warmed—it was smooth and unctuous to the touch.

2. The specific gravity of two different portions, was as 1630 and 1633, that of distilled water being 1580; each substance being of the same temperature. Serum of the blood of different patients, was found at the same time to be 1626, 1627, and 1630. Accordingly, the distilled water being 1000, the pus is 1031, and 1033; and the serum is 1029, and 1031.

3. After 12 hours repose, about two ounces by measure of a limpid fluid having appeared on the top, it was decanted from off the opaque purulent fluid; which was become thinner in the upper part of the vessel containing it, and thicker in the lower than before.

4. On further repose, it did not become offensive so soon as a portion of the same pus mixed with a little blood, nor as serum alone.

5. This pus neither indicated acidity nor alkalescency to the usual tests, viz. turnsole paper, tincture of red cabbage, Brazil-wood paper, and turmeric paper. I have, in other instances sometimes, observed acidity to be indicated by turnsole paper; but in none alkalescency, so long as the matter remained without fœtor.

6. Being examined under the microscope, when duly diluted with distilled water, innumerable spherical particles were seen, which did not appear altered in figure, nor diminish in number by extreme dilution; that is, they did not appear to have been dissolved.

II. A pint of pus of the second kind, *viz. curdy*, was afforded by a psoas abscess.

The colour was brown. It felt knotty. On pouring from

one vessel to another, the curdy masses were manifest, and of various sizes, from that of a pin's head to a hazel nut. It was more viscid than the former, and of a little greater specific gravity. On standing, a limpid fluid appeared upon the top, as in the first kind, but in smaller quantity. Globules were seen with the microscope, but also a number of irregularly figured larger masses. Putrefaction took place sooner than in the former kind. In other properties, this pus was similar to the first kind.

III. *Serous thin pus.* It was produced by a fatal inflammation of the peritoneal coat, without ulcer, and taken out of the cavity of the abdomen. A good deal of serum was also effused, of which the pus was a deposit. It was not much thicker than milk. To the feeling it was not at all unctuous. The smell was slightly offensive. On standing 24 hours a sediment appeared, occupying only one half the full vessel, under a whey-like liquid. Putrefaction took place sooner than in either of the two former kinds. The specific gravity was the same as that of the first sort. In other properties it was similar to the cream-like pus above distinguished.

IV. A pint of the *viscid pus* was obtained from an abscess among the muscles of the thigh. If I had not had entire confidence in Mr. BRODIE's accuracy, who was so obliging as to attend to my request, on this and many other like occasions, I should have supposed, that this was expectorated matter, it so exactly resembled in its simple properties, the *ropy kind*, described in a paper on expectorated matter. Phil. Trans. 1809, P. II. p. 317.

The appearance was not quite uniform, there being semi-transparent masses in small proportion, mixed with the

perfectly opaque white matter. It was almost inodorous. To the touch it was quite smooth. The specific gravity was nearly that of the second kind of pus.

On standing 24 hours, about one ounce measure of limpid fluid rose to the top of the whole mass. Putrefaction did not take place so soon as in expectorated matter of the same consistence.

The examination by the microscope manifested innumerable spherical particles among leafy masses, and numerous particles of irregular forms.

The simple properties were otherwise similar to those of the other sorts of pus, above distinguished.

Many other differences of purulent matter are universally recognized; but they are either varieties of the four kinds already named, or the differences depend upon the obvious mixture with adventitious substances; such as the red part of the blood, coagulated lymph, serum, putrefied matter, fibrous and membranous masses, calculi, &c.: therefore, I deem it useless to describe them.

Sect. II. *Agency of Caloric.*

1. The above kinds of pus coagulated like serum of blood, into a firm, uniform, soft solid, at the temperature of 165° completely; but partially at 160° of Fahrenheit's thermometer.

2. The decanted limpid fluid from pus, Sect. I.—I. II. III. IV. coagulated completely into a firm uniform mass, like serum of blood, at 165°, but it became opaque and thickened at 160°. By pressure of the firm curd thus produced, a watery liquid was separated, which on due evaporation did not give a jelly, but was coagulable like the decanted liquid just mentioned.

The thick opaque matter, after decanting the limpid fluid, coagulated as before said, into a firm mass at 165°.

3. Each of the above four kinds of pus being evaporated to dryness, left in no case less than one tenth of its original weight, nor more than one sixth; but most frequently one seventh, to one eighth of brittle matter. The smallest proportion of residue was left by the 3d, or serous kind; the largest, by the 2d or curdy. These residues generally became rather soft, especially those of the 3d, or the serous kind, after exposure to the air.

4. The opaque part of pus after separating the limpid fluid afforded on evaporation about $\frac{1}{15}$ to $\frac{1}{30}$ more of brittle residue, than an equal weight of the pus itself; and it remained hard on exposure to the air. The limpid fluid evaporated to dryness, yielded about one tenth of brittle residue; which grew moist, and sometimes deliquesced, on exposure to the air.

5. The brittle residues above mentioned (3), being exposed to fire in platina crucibles, flamed for some time, emitting a very offensive, pungent, empyreumatic smell; the uninflammable residue being kept in a state of ignition for a longer period, what remained at length was fused readily from the *serous*, *viz.* the third kind of pus; but in the cases of the other exsiccated residues of the 1st, 2d, and 4th kinds of pus, they barely were melted, or only became soft and claggy. The fused residues from the *serous pus*, amounted to $\frac{1}{30}$ to $\frac{2}{31}$ of the exsiccated pus; and to $\frac{1}{250}$ to $\frac{1}{300}$ of the original purulent matter. Those from the second kind, the *curdy*, amounted to $\frac{1}{50}$ to $\frac{2}{60}$ of the dried matter, and to $\frac{1}{350}$ to $\frac{1}{400}$ of the pus itself. The fused masses from the 1st and 4th kinds of purulent matter, afforded intermediate quantities of melted matter between those just mentioned.

6. The fused residues (5), being treated in the manner described in a former paper, Phil. Trans. 1809, P. II. p. 326—329, I found they consisted chiefly of muriate of soda, phosphate of lime, and potash; with strong indications of carbonate of lime, and a sulphate; besides traces of phosphate of magnesia, oxide of iron, and vitrifiable matter, probably silica. On a reasonable calculation, it appeared, that in the *serous kind* of pus, the muriate of soda amounts to from one and a half, to two per 1000; the phosphate of lime to one, to one and a half per 1000; the potash to one half, to three fourths of a part in this quantity; and the other matters together, to half a part in 1000. In the *curdy matter*, the second kind, the muriate of soda amounts to three fourths of a part, to one in a 1000; the phosphate of lime to one; the potash to less than one half; and the other matters united, to half a part in a 1000. The first kind of pus, the *cream-like*, and the fourth, the *viscid*, afforded from the melted residue, the same substances as the *serous* kind, excepting a somewhat smaller proportion of muriate of soda, and potash.

7. The brittle residues of evaporated pus, after decanting the limpid fluid (4), being treated with fire as above related, the remaining matters were melted with more difficulty, and less completely, and contained a smaller proportion of muriate of soda and potash than the original pus.

8. The decanted limpid fluids (4), being evaporated to dryness, these residues were exposed to fire. They were melted, and then afforded a larger proportion of muriate of soda and of potash, than the pus itself; but with the same proportion of the other saline and earthy substances.

SECT. III. *Agency of Water.*

1. After decanting the limpid fluid from off half a pint of the four kinds of pus as above related, (Sect. I.) three ounces measure of distilled water were mixed with each of them. After 48 hours repose, a limpid fluid of nearly the quantity of two ounces by measure, was seen forming an upper stratum to the pus. It was decanted for examination.

(*a*) On exposure to fire it became turbid like milk, as soon as the temperature was elevated to 165°, but did not become thicker at a greater elevation.

(*b*) On evaporation to dryness, the residue amounted to about one fifteenth of the weight of the liquid from the serous pus, and to one twentieth from the three other kinds; in place of about one tenth, as from the first decanted liquid, (Sect. I. 4); and as from serum of blood. The residuary matters were of the same kind as those above described, Sect. II. 2—6.

(*c*) Three ounces measure of distilled water having been again mixed with each of the four kinds of pus, and in 48 hours, two ounces measure of decanted limpid fluid from each, having been evaporated to dryness, residues of the same kind, in the same proportions, and in nearly the same quantities as before, were obtained (*b*). These decanted fluids became nearly as turbid as the former, on raising their temperature to 165°.

(*d*) Distilled water was added a third time in the quantity of eight ounces by measure, to each of the four parcels of pus under examination, and after 48 hours repose, six ounces of limpid fluid were poured off from each of them. At the temperature of 165°, the decanted fluids became turbid; that of the serous pus more so than the others. On evaporation to dryness,

a much smaller quantity of residue was obtained than before, viz. one sixtieth from the serous pus, and one seventieth from the others; and it consisted of the same kind of substances as above described; but the muriate of soda and potash were in smaller proportion than before.

(*e*) A fourth time distilled water in the quantity of a pint, was mixed with the present four parcels of pus, and after standing 48 hours, three fourths of a pint of clear colourless liquid was poured off from each of them. It became slightly turbid and whitish on boiling. On evaporation, each parcel afforded about $\frac{1}{90}$ of the fluid employed. The residues now consisted of animal matter, with a much smaller proportion than before, of muriate of soda, phosphate of lime, and potash—nothing else could now be traced.

(*f*) Distilled water in the quantity of a pint, was once more mixed with the four sorts of purulent matter undergoing inquiry. After 48 hours, a pint of liquid was decanted from off each of them; but being slightly turbid, they were left to stand 24 hours. By this time a sediment was deposited from each of the liquors; but being still, though very slightly, turbid, they were filtrated through suitable paper. They were then transparent. The transparent filtrated liquors had their transparency disturbed by a boiling temperature. They became also slightly milky, with nitrate of silver, but scarcely so with infusion of gall nut. On evaporation to the quantity of an ounce from each pint, the residuary liquids appeared slightly globular. These, on evaporation to dryness, yielded not more than one part of animal matter, from each 500 of the transparent filtrated liquids.

(*g*) On standing three or four days in a cold room, the

parcels of pus, after the ablutions just related (*a*—*f*), exhibited a whey coloured liquor at the top, of which about $\frac{1}{4}$ of a pint was poured off from them. More turbid liquor was also separated from the washed pus, by pouring it upon a porous cotton cloth strainer, which left purulent matter of the consistence of starch mucilage, amounting to about one half the original weight.

(*h*) The pus freed from coagulable limpid liquid by repeated ablutions (*a*—*h*) was white as snow—equal in consistence—perfectly smooth—the 4th kind was less viscid than before, but the others were more so—no smell—not at all disposed to putrefy—on elevating its temperature to 165° and higher, it did not coagulate into one mass, nor into clots, or large masses of curd, but a watery fluid separated from a fine soft somewhat curd-like opaque fluid; which did not become more curdy, on even boiling—it did not appear that above a grain of this part, or state of pus, dissolved in 1000 waters—was highly globular under the microscope, and remained so, although coagulated by nitrate of silver; by infusion of gall nut; by alcohol; and super-sulphate of alumina—with muriate of ammonia, nitrate of potash, and other neutral salts, and with carbonate of potash, it produced a viscid semi-transparent mass like expectorated half transparent matter—exposed to fire in a platina crucible, it was inflamed, but did not emit an offensive smell, and after continuing the ignition, the residue was a particle of half fused matter, not amounting to $\frac{1}{3000}$ of the pus after ablution, nor above $\frac{1}{800}$ of the same matter exsiccated; it consisted of phosphate of lime and vitrified matter—no ammonia was perceivable, on mixing lime with this washed pus; nor muriatic acid on adding sulphuric acid.

2. (*a*) A tea spoonful of the *cream-like pus*, being agitated in

half a pint of distilled water, produced a milky fluid, with a number of small curdy particles suspended, but very few leafy or fibrous pieces or clots.

(*b*) The *serous pus* being treated as just mentioned (*a*), the same appearances ensued.

(*c*) The *curdy pus* being agitated in the same manner in water, a number of clots, leafy, and fibrous masses, were seen suspended among fine small curdy particles in a pearly liquid.

(*d*) The *viscid pus* being treated as just said, it required long continued, and violent agitation, to diffuse it through the water, and then the appearances were as last described.

3. Pus of any kind, after boiling in twenty times its quantity of water, was quite as globular under the microscope as previously. With a smaller proportion of water, the mixture became very turbid, sometimes clots were formed in a pearl liquid, in which a fine sediment took place, which appeared much more globular than the clots or curdy masses.

4. In general, water in which pus has been agitated, remains somewhat milky, with an abundant close white sediment; but after two, or three, or more ablutions, the water becomes clear on standing, and the sediment more curdy.

Sect. IV. *Agency of Alcohol of Wine.*

The different kinds of exsiccated pus exposed to the agency of this menstruum, and treated as described in a former paper, Phil. Trans. 1809, P. II. p. 329, the results were similar, except in the proportion of products.

1. These exsiccated substances afforded to this menstruum a smaller proportion of potash, but as much animal oxide and muriate of soda, as mucous sputum.

2. The undissolved matter left after repeated digestions in this menstruum, afforded the same substances, but in smaller proportions, as mucous sputum.

3. Equal bulks of fresh pus, and rectified spirit of wine, afford a much thicker and more milky liquor, with a closer sediment, than expectorated mucous matter.

SECT. V. *Agency of acetous Acid.*

The purulent matters mixed with this acid became curdy, and rendered it milky; but on standing, a close white sediment appeared, the liquid above being clear, except in the case of the viscid pus, which exhibited leafy and fibrous masses, as hath been described with mucous sputum.

By repeated digestion of the different kinds of pus in this menstruum, I obtained the same results, except the proportions of acetite of potash, and muriate of soda being smaller, as related in a former paper on mucous expectorated matter. Phil. Trans. 1809, P. II. p. 336.

SECT. VI.—*Some Experiments with different Objects, especially to distinguish Pus and Mucus.*

1. In the agency of sulphuric, nitric, and muriatic acids, in sufficient quantity to dissolve and decompound the substances under inquiry, I could perceive no important difference between them. The purulent matters indeed, required a much greater proportion to completely dissolve them, than the transparent sputum. Also the more opaque and dense the sputum, the greater the resistance to dissolution. Sulphuric acid produced black liquids like those containing charcoal, smelling strongly of muriatic acid, but on dilution with water, they became clear. No precipitation occurred on dilution with water, and on

saturation with the fixed alkalies, but a trifling sediment appeared, which re-dissolved on the addition of the above acids.

2. The mineral acids diluted, or added in small proportion, and the vegetable acids, coagulate variously pus and mucous fluids. Some become merely milky fluids, others curdy fluids, others afford fibrous and leafy masses in a transparent liquor, and others give an uniform thick mass of curd. On standing the deposits are accordingly of various forms, and the liquors above of various appearances, but I could discover no constant characteristic property of the substances by these experiments, as some writers have asserted.

3. The solid fixed alkalies, or lime, mixed with expectorated mucus, occasion a stronger smell of ammonia than with pus; or than with muco-purulent sputum. Some use may be perhaps made of this easy experiment to judge of the nature of varieties of the fluids in question, particularly as far as depends on the proportion of ammonia: for sometimes it cannot be perceived by the smell on mixing alkalies, but can by muriatic acid giving white vapours. Concentrated liquid alkalies, added to both pus and mucus, dissolve them to produce clear liquids, except small curdy parts and motes. These curdy parts and motes resist dissolution also for some time even in nitric acid, and seem to be self-coagulated lymph. They are in much greater proportion in pus than mucus. The addition of acids to these alkaline dissolutions, occasions precipitations, but no differences, or not with sufficient uniformity to afford criteria, were observed according to the observations of other Experimenters.

4. Concentrated aqueous solutions of various neutral salts, *viz.* muriate of ammonia; nitrate of potash; muriate of soda;

sulphate of soda, &c. being mixed in due quantity with pus of the kinds under examination produce viscidity, like ropy expectorated matter, thickening like jelly, and less opacity. These changes have, in the case of muriate of ammonia, been called coagulation by Mr. HUNTER; but by agitation in cold water the matters are diffused, and on standing, the pus is precipitated in its original state. I call these effects of the neutral salts inspissation, seemingly occasioned by their attracting water from the pus; for no such change is produced if either the purulent matter, or solution of salts be diluted; nor is it produced if the pus be previously coagulated by caloric: also the inspissated pus is coagulable by caloric as usual. No such inspissation is produced by these salts in mucous sputum, nor in muco-purulent sputum, so that undoubtedly it is a criterion as discovered by Mr. HUNTER in the case of muriate of ammonia, and with other neutral salts, as now manifested.

4. I endeavoured to find some easy tests for distinguishing pus from mucus; but I did not succeed with the tanning principle; gallic acid; super sulphate of alumina; nitrate of silver, and other metallic salts; and as already said, various acids. They all produced precipitation of these animal matters, but not with observable characteristic differences.

5. To observe the state in which the matter of pus is secreted, I procured the assistance of Mr. MAYNARD, the present house-surgeon of St. George's hospital, and Mr. GEORGE EWBANK, who had been on many occasions essentially serviceable in my inquiries. Square pieces of gold beater's skin were applied to various sore legs after carefully removing the matter already secreted. In five to ten minutes

the square pieces being removed, were found wet with a limpid fluid. In this state they were inspected by the microscope, by which numerous globules were seen. In ten minutes further the liquid was no longer limpid but opaque like pus, in which the usual spherical particles were seen with the microscope as just mentioned.

Supposing objections might be offered on account of the alteration of texture of the skin employed, square pieces of glass were also applied. The results were the same in both trials. The two gentlemen above named, as well as Dr. Richard Harrison, and other pupils, who happened to be present, all concurred in the observation, that the limpid matter became opaque, and that while limpid it was, like pus, full of spherical particles.

Sect. VII. *Conclusions.*

The statement of the properties of pus in the foregoing inquiry I hope will be found to be true; and I submit to the judgment of others whether or no the following inferences are legitimately established.

1. That this fluid essentially consists of three distinct substances, viz. 1. An animal oxide, which, among other properties, is distinguished by its being white, opaque, smooth, of the form of fine curdy particles in water; not dissoluble in less than 1000 cold waters; not coagulable into one mass like serum of blood by caloric, alcohol, &c.; only rendered more curdy by water of 160° to 170°; but readily diffusible.—2. A limpid fluid resembling serum of blood in its impregnations and in its coagulability by caloric, alcohol, &c.; in which the opaque oxide is diffusible but not dissoluble, and which is specifically lighter than that oxide.—3. Innumerable spherical

particles visible only by the microscope in this opaque oxide, and in small number in the limpid fluid; not coagulable by any temperature to which hitherto exposed, and not destructible by many things which combine or destroy the opaque oxide; and these globules are specifically heavier than water.*

2. That the *visible* curdy masses, as well as the fibrous or leafy parts, almost always contained in smaller or larger quantities in pus, may be considered as self-coagulated lymph, which in its fluid state is secreted without having the state of aggregation produced in it like that of the *essential* opaque oxide of pus.—Sect. VII. 1.

3. That the reddish, the blackish, and the dark brown colour of pus depends upon the red part of the blood effused or secreted from the same vessels, or from contiguous ones which secrete pus.

4. That on some occasions the clotty and irregularly figured masses found in the pus may depend upon disorganization or breach of the contiguous solid parts.

5. That whenever pus is fœtid to the smell, a portion of it is in the state of putrefactive fermentation, which may be removed by ablutions with water.

6. That there are certain adventitious matters liable to be contained in pus not hitherto rendered palpable to the senses, but known by their effects in exciting contagious diseases; such as small-pox, syphilis, &c. These matters are produced by a specific action in the secretory organs of pus, by such

* My obligingly attentive pupils, Mr. BURTON, and Mr. STANSFELD, house-surgeons of the Lock hospital, collected for me a sufficient quantity of gonorrheal matter to determine that it consisted of the three ingredients here stated.

matters themselves either contained in the circulating blood, or on the secreting surface.

7. That the *essential* substances of which pus consists, as well as some of the adventitious ones (Sect. VII. 1, 2, 3, 6), are separated from the blood by a peculiar organization belonging, or attached to the blood-vessels: which organs of separation or secretion are not only excited to the action which produces pus in diseased states, but they are evidently influenced by the states of other distant organs of the animal œconomy; hence many varieties in the properties of the purulent matter.

8. That the varieties of purulent matter relate to differences of *quantity*—the proportion of the essential substances (1)—and the adventitious parts (2, 3, 4, 5, 6,). The *cream-like* pus consisting of almost purely the opaque oxide and limpid liquid (I. 1, 2). The *curdy* containing a large proportion of coagulated lymph, or broken down solids. The *serous* abounding in limpid fluid. The *viscid* depending upon the coagulation, and perhaps, inspissation, by union of neutral salts with the opaque oxide.

9. That as the essential parts are secreted in a limpid state, but presently become opaque, owing to a large proportion spontaneously coagulating, and thus becoming the opaque oxide, mixed with the serous liquid, and innumerable spherical particles (Sect. VII. I. 1, 2, 3), it seems reasonable to infer that these matters are the self-coagulated lymph of the blood and serum, separated by the secretory organs; which act of secretion determines the subsequent state of aggregation of pus, and the globules are at the same time formed analogously to their formation by other secretory organs. How far they are

those of the blood altered by secretion may be determined hereafter. It is a collateral proof of this inference that very thick pus affords one sixth to one seventh of exsiccated brittle residue, which, as I have found, is nearly the same proportion afforded on the exsiccation of the buffy coat of inflamed blood; while very thin pus affords on exsiccation one eighth to one eleventh of brittle residue, which is the proportion to be expected from a mixture of serum of blood and self-coagulated lymph, as I have ascertained.

10. That the constant impregnating saline and earthy ingredients of pus, are dissolved in the serous fluid, and are all separable along with the serum, by ablutions with water, from the opaque oxide (1), except a portion of the phosphate of lime. These impregnations are the same as those of serum of blood, and of expectorated mucous matter, *viz.* muriate of soda; potash neutralized by animal matter or a destructible acid: phosphate of lime; ammonia neutralized probably by phosphoric acid; with a sulphate and traces of some other matters mentioned in my former paper. The proportion of these impregnating substances is as the proportion of limpid or serous coagulable fluid, and of course inversely as the proportion of the opaque oxide of pus; but it varies in different cases in given proportions of this oxide, and the limpid fluid. In general, if not always, a given quantity of pus contains a smaller proportion of saline matters than an equal given quantity of expectorated mucous matter, but a given quantity of the limpid coagulable fluid contains a greater proportion of saline matters than an equal given quantity of serum of blood. Hence the thicker the pus the less irritation to the sore which secretes it, and commonly the less the inflammatory or other

action of the secreting surface. In different cases, however, the proportion of impregnating saline substances to one another is liable to vary, especially that of phosphate of lime; hence, though rarely, calculi occur of this substance in the cavity of the abscess.* Hence too the exsiccated pus is liable to become soft and moist, from the proportion of neutralized potash being greater than usual; and even deliquescence sometimes occurs of the exsiccated limpid fluid.

12. That the same organs, according to their different states, secrete from the blood merely water impregnated with the saline substances of the serum of blood; also this fluid containing various proportions of coagulable matter like that of serum of blood; and serous fluid with self-coagulable lymph, which affords curdy masses: likewise this serous fluid, together with this matter which coagulates of itself after secretion, highly impregnated with invisibly small particles, in such a state of aggregation, as to constitute the thick opaque fluid called pus—which states of the secretory organs are generally attended with inflammatory action, but frequently also without any symptoms of such action.

13. That besides the consistence of pus depending upon the proportion of serous limpid liquid, and opaque matter, it also probably depends upon the mode and state of coagulation of the matter which affords this opaque part; analogously to the different states of consistence of the coagulated blood

* On examining the lungs of a patient who died of pulmonary consumption, concretions were found in a large vomica from the size of mustard seed to a pepper corn, which Dr. E. N. Bancroft reserved for my enquiry. I found they consisted chiefly of phosphate of lime with an unusually small proportion of animal matter. In another patient of Dr. Nevinson, matter was coughed up, consisting chiefly of phosphate of lime and animal matter, nearly one of the former to three of the latter.

itself, according to the different conditions of the animal œconomy.

According to the above inferences, I trust, a distinct and definite notion of the substance to be considered as pus is exhibited; and I do not comment on the different results of experiments and conclusions of other writers, because future observers only can determine the truth. What is and what is not pus will now readily be ascertained by a few easy experiments; by the obvious properties; and by the consideration of the source of the matter in question: provided, however, that it be unmixed with certain other matters by which disguise is produced. As already observed it is in pulmonic diseases that the ambiguity occurs; and physicians lay very considerable stress upon the nature of expectorated matter in their practice and reasoning; I shall therefore endeavour to elucidate the subject by remarks on the puriform matter expectorated in different cases.

1. An abscess occasioned by acute inflammation not only of a pleurisy, and peripneumony, but of other diseases which have not the symptoms of any one which has received a designation. Here there ought to be no doubt; for the matter which is coughed up suddenly and abundantly on the bursting of the abscess is evidently pus with little mucus. Such matter consists of the essential ingredients of pus, (Sect. VII. 1,) with generally the adventitious substances, (Sect. VII. 2, 3, 4.) *viz.* coagulated lymph, membranous or fibrous parts, and a small proportion of the red part of blood.

2. Purulent expectoration from the rupture of abscesses, or vomicæ of suppurated tubercles. In such cases there has been a chronical cough with viscid sputum, commonly in persons of

an advanced age. After this long continued disease, an abundant expectoration of quite a different kind from the former suddenly comes on; by which the patient often dies very speedily; sometimes immediately, being seemingly choaked. This kind of matter evidently consists chiefly of the essential ingredients of pus (Sect. VII. 1,) with not only the adventitious substances, *viz.* clots of self-coagulated lymph, and sometimes the red part of blood, but also masses, which are apparently the broken down solid parts, the cellular membrane, the vessels, and substance of the tubercles, in a disorganized state. The sufferer often says, such matter tastes sweet. The mucus is here in too small a proportion, and not intimately mixed, to occasion disguise.

3. In the bronchitis, or inflammatory affection of the air tubes, the membrane remaining entire, attending various diseases, e. g. the measles, a fever with a cold, various continued fevers, an expectoration of thin cream-like matter occurs, at first gradually, but at last in great quantities, continuing for a week or more. Although mucus is usually coughed up with this puriform substance, the two things generally remain in distinctly large masses. With little skill, the opaque or puriform fluid may be collected separately from the mucous matter. It will be found to consist almost purely of the three essential constituents of pus (Sect. VII. 1,) there being seldom any adventitious substances.

4. Muco-purulent, or commixed expectorated matter. This kind is perhaps of the most frequent occurrence. It is that which many physicians know not how to designate, some consider it to be pus, and others to be mucous matter. This contrariety of opinion arises from the want of definite notions of

pus and mucus. Hence the parties are not able to perceive that in this kind of sputum, exist many of the properties of pus, and also of mucus. I have described it in my former paper on expectorated matter, Phil. Trans. 1809, P. II. p. 317, under the denomination of *opaque ropy matter*, the third kind. I feel no degradation in finding it necessary to confess, that a better acquaintance with the properties of pus, has taught me that I was in an error, in considering this kind of expectorated matter to differ from other sorts, merely in the proportion, and not in the kinds of constituent parts. It now appears, that the sputum in question possesses such properties as might be predicted to exist, from the known properties of pus and mucus separately, in case these two substances should be intimately commixed. Accordingly, the opacity; the straw colour; the greater density than mucus; the great globularity under the microscope; the greater proportion of residue on evaporation to dryness, than from mucus; the milky liquid on heating this matter; the milkiness on agitation in cold water; are properties of pus. But the great viscidity, yet not increased by neutral salts; the less opacity than pus; the less globularity than pus; the smaller proportion of exsiccated residue than from pus; the moisture, or greater moisture on the exposure of the brittle residue to air, than from that of pus; the more difficult diffusibility through cold water, and less degree of milkiness than from pus: the great proportion of leafy or fibrous masses on agitation in a very large quantity of cold water; the speedy putrescency; are properties of mucus. The mode of coagulation by caloric at 160° and upwards, is such as might be expected from the commixture, *viz.* in large masses of curd in a milky liquid, instead of into one uniform

mass like pus, or into small curdy masses in a very large proportion of whey coloured liquid, like mucous sputum. Thick pus affords on evaporation to brittleness, $\frac{1}{7}$ or $\frac{1}{8}$ residue; and transparent sputum of the consistence of jelly, gives about $\frac{1}{16}$ or $\frac{1}{20}$ of such residue; but this opaque matter under inquiry, affords $\frac{1}{10}$ to $\frac{1}{15}$ of brittle residue, according to the proportion of the two substances. I could not separate the supposed pus and mucus from one another, to exhibit them distinctly by water, or by any other means, on account, as I conceive, of the intimate diffusion through one another, and their mutual cohesion. But on evaporating the milky water, produced by agitating this sputum in it, or by letting it stand to collect the sediment, little else beside a mere congeries of globules seen under the microscope, was thus obtained. For the same reason, on standing, a serous liquid like that of pus (Sect. VII. 1) does not separate, or only partially, from the opaque part, so as to render it possible by ablution, to collect this coagulable liquid like that of pus: and the greater proportion of water, belonging to the mucus, occasions the coagulation by caloric, to afford only a milky liquid, instead of an uniform mass of curd.

This kind of sputum, consistently with the phenomena, must be produced by secretion from the bronchial membrane in its entire state, and not by ulceration or abscess. For it is secreted in many cases, at the rate of a pint or more in each 24 hours, for weeks and months successively, and for 20 or more successive winters. Also many persons recover their good health after this secretion, and it is the usual termination favourably of pneumonia, bronchitis, &c. It is produced by any disease of great irritation of the lungs; as I have found

from ossification of the bronchial or pulmonary arteries; from calculi; from broken wind, or rupture of air cells, &c.*

It is secreted also in consequence of irritation of the bronchial membrane by tubercles, vomicæ, water in the cavities of the chest, &c. The same kind of matter is secreted from the nose on the decline of a common severe coryza in many cases. It appears then, that this kind of matter is a symptom of the most fatal, as well as harmless diseases—it is a symptom in one case, of the progress of disease to death, and in another, of the termination in health, by being seemingly a critical discharge. Perhaps, if these facts had been observed and considered, numerous mistakes in prognostics, would have been avoided, and better practice have been employed; because the nature of diseases would have been rightly understood. From this representation, it is plain, that a just opinion cannot be given merely from the examination of the sputum, without considering the disease by which it is produced, or of which it is a symptom.

The proportion must also be considered of the pus and mucus in sputum: it may be estimated, by attending to the properties of each, as above stated.

Such a compound as the present, scarcely is produced in any other part, but in the bronchial, and mucous membrane of the nose, because of the abundant secretion of mucus from these membranes. And when it is conceived, that both pus and mucus are secreted in a limpid state, from the same or at least contiguous organs, where they first intimately commix, and then become inspissated; it will appear reasonable, that they

* I believe this state of the lungs to have been first ascertained in broken-winded horses, by Mr. COLMAN.

cannot be readily, or at all completely separated again from one another. There is indeed, in these cases, no necessity for the admission of the secretion of the limpid fluid of pus of abscesses (Sect. VII. 1); for it appears to me not unjust to consider mucus to be nothing more than the serum of blood, altered in its composition and proportion of water, so as to produce a viscid texture. The secretory organs of the mucous membrane, by virtue of their peculiar power, separate from the blood, in health, the mucus as above said, with some globules, and also a small proportion of the self-coagulable lymph; which appears, on agitating mucus in a large proportion of cold water, in the form of leafy and fibrous masses.* The same secretory organs, it is easily conceivable, may in a diseased state, be excited to separate also self-coagulable matter from the blood, with more globules, in such a state as to become pus. Hence, such a commixture of the two substances must correspond to the opaque, viscid, expectorated sputum, of which I am writing.

If I thought farther reasoning proper, it would be manifest, that all the phenomena, both in health and disease, belonging to the various kinds of sputum, consist with the theory above delivered.

* Serum of blood appears always to contain self-coagulable lymph, which is deposited on standing; and this appearance led GABER, PRINGLE, and CULLEN, into the erroneous opinion, of this deposit being pus itself.

ERRATA.

P. 103, second line of the note, *for* 24.16 *read* 241.6.

P. 121, fourth line, *for* pan *read* from.

P. 253, line 21, *for* potash *read* potassium.

MAGNETIC NEEDLE,

June, 1809.

Variation - - - 24° 11′.

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